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ENIGMAS:

ANOTHER BOOK OF UNEXPLAINED FACTS

BY THE SAME AUTHOR

THE MARINE CHRONOMETER: ITS HISTORY
AND DEVELOPMENT

ODDITIES: A BOOK OF UNEXPLAINED FACTS



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MEMNON (right-hand figure)

G. H. Hall

ENIGMAS

ANOTHER BOOK OF UNEXPLAINED FACTS

By

LIEUT.-COMMANDER

RUPERT T. GOULD, R.N. (RETIRED).



✓
LONDON

Folklore

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TO
MY MOTHER

TO THE READER, PLEADING EXTENUATING CIRCUMSTANCES

I do not think that I can explain the intention of this book more clearly than by quoting from the Preface to its predecessor, *Oddities*:

“The essays contained in this book, although apparently disconnected, were written at one time and with one object—to collect and digest the facts relating to a number of incidents which have not, at present, been satisfactorily explained.

“In order to present these facts as clearly as possible, I have quoted rather extensively from the original sources. As most of these are not very accessible, I hope that I may be spared the reproach that the book has mainly been put together with scissors and paste.”

Enigmas differs slightly from *Oddities* in scope, but the method pursued has been the same: to present the facts, wherever possible, in the form of quotations from the original sources, and to supply enough references to enable any one who will to go into them more deeply.

With one exception, the present essays have not previously been published. “The Landfall of Columbus” was read before the Royal Geographical Society in February 1927, and appeared in the *Geographical Journal* for the following May. I have to thank the Council of the Royal Geographical Society for permission to reprint it here. I should like also to express my thanks to the authorities of the Royal Society, the Royal Astronomical

Society, and the Admiralty Library for much information freely placed at my disposal; and to many friends and correspondents, particularly Mr. E. Heron Allen, F.R.S., the Rev. T. E. R. Phillips, and Mr. R. Humphrey Eggar.

RUPERT T. GOULD.

ASSTEAD, 1929.

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ENIGMAS

THERE WERE GIANTS IN THOSE DAYS

IN the year 1577 a skeleton of enormous size was discovered at Willisau, in the canton of Lucerne, Switzerland. The local authorities, undecided as to the nature of the remains, which were incomplete, and doubtful whether (as in the famous case of the Peruvian mummy left unclaimed in the parcels-office of a London station*) they ought not to hold an inquest and/or give them Christian burial, consulted the famous Dr. Felix Plater, of Basle, the most expert anatomist (strange though it may seem) of his day. Plater gave it as his opinion that the bones were undoubtedly human, and forwarded to Lucerne an anatomical drawing of their original owner, who must (according to this) have stood some nineteen feet high.† The "Giant of Lucerne" lay in state at the town hall of Lucerne until, in an evil hour, he was visited by a still more competent anatomist than Plater—one J. F. Blumenbach (1752–1840), of Göttingen, a savant possessed in full or even brimming measure of that irritating turn of mind which takes nothing for granted. One glance at the bones was enough for him—

* This inquest was actually held, and the very reasonable verdict returned that the deceased had come by his death so long ago that the cause of it could not be ascertained. As the law then stood (it has since been altered), the holding of an inquest, even in such ridiculous circumstances, was imperative.

† See Fig. 1.

but it was no easy matter to convince the good citizens of Lucerne, who had adopted the Giant as one of the supporters of their city arms, that he was only a mammoth.

Giants of this kind had a habit of turning up, during the Middle Ages and even later, in all parts of Europe. For example, in 1613 the learned world was astounded to hear of a giant found by some workmen in a sandpit

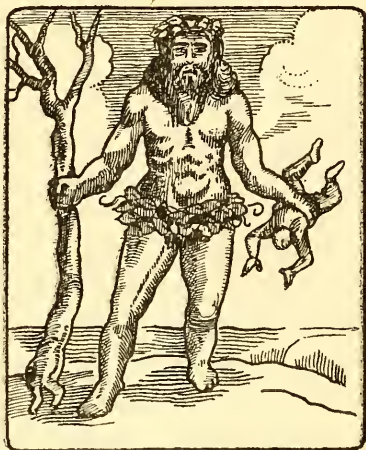


FIG. 1.—The Giant of Lucerne. Re-drawn from an engraving in J. L. Cysat's *Beschreibung dess berühmten Lucerner . . .* (Lucerne, 1661).

NOTE.—The engraving represents a painting by Johann Bock (after a drawing by Felix Plater) presented to the Senate of Lucerne in July, 1584.

near the castle of Chaumont, not far from St. Antoine. At a depth of 18 feet they discovered a brick tomb 30 feet long, 12 feet wide, and 8 feet high. In this lay an entire human skeleton, over 25 feet in length, 10 feet across the shoulders,* and 5 feet deep from the breast to the back. Furthermore, an inscription in Gothic characters on the lid of the tomb read "Teutobochtus

* This, in itself, was suspicious. The breadth of the shoulders in man is generally not much more than a quarter of the height.

Rex," and demonstrated, in conjunction with medals, coins, etc., found near by, that the body was that of Teutobochtus, the giant king of the Cimbri, who was defeated and captured by Marius, near Aix, in Provence; and whose head, as he walked behind his conqueror's triumphal car, is recorded by Florus* to have overtopped the trophies carried on the Roman spears (as well it might). The details of this truly amazing find were vouched for by one Dr. Mazurier,† a surgeon, who, shortly afterwards, issued a pamphlet about them—the first shot of a long battle.

The genuineness of Mazurier's account was violently attacked; notably by one Jean Riolan, a celebrated naturalist of the period, in two tracts with the snappy little titles of *Gigantomachia* and *Gigantologie*. In all, over a dozen pamphlets were hurled to and fro. Finally, it was made clear that Mazurier had bought from the workmen various enormous bones which they had found in the pit, and had supplied the rest of the details himself. Teutobochtus, restored to his original configuration—that of a mastodon—is now in the Musée de Paléontologie, Paris.

In the time of James I, certain "big, outlandish bones" were discovered at Gloucester, and Lord Herbert of Cherbury was appointed to find out what he could about them. He was assisted (or, perhaps, impeded) by several pundits, some of whom advised the re-interment of the bones with Christian rites; while others, including the great William Harvey, declared them to belong to "some exceeding great beast, as an elephant." One of them, Bishop Hakewell, remarks:

* In his *Epitome de Gestis Romanorum*.

† He seems to have used the pseudonym "Nicholas Habicot." Can Voltaire have confused him with his ". . . coquin d'Habacuc, capable de tout" ?

"His Lordship showed me some bones, which he had collected; which were a huckle-bone,* part of the shoulder-blade, some parts of a tooth, and the bridge of a nose—all of a huge bigness. . . . The bridge of the nose was what confirmed his lordship's and my opinion, that it could not be that of a man, for it did seem to be a bone very apt to bear up the long snout of an elephant. One of the teeth of this pretended giant, by the special favour of my lord of Gloucester, was examined by me. I found it to be a stony substance, both for hardness and weight; and it should seem, by his lordship's letter to me, that he himself was not confident that it was the tooth of a man."

As Greenwood† has pointed out, in those days simple arithmetic seems to have been as unknown a science as comparative anatomy. The tooth in question, if that of an elephant (which, no doubt, it was) would have weighed some ten pounds. Human teeth scale about 160 to the pound; and in the same proportion the owner of the tooth, if he were human, would have weighed about a hundred tons.

But, while such blunders seem almost incredible to-day, there are two potent factors to be borne in mind. Until the very end of the eighteenth century it was generally believed that the fauna of a given locality must have persisted, unchanged, since the Creation; and when, for example, Cuvier first announced the discovery of elephant, hippopotamus, and rhinoceros remains in the upper European strata, he was gravely informed that these must belong to the elephants brought from India by Pyrrhus.

And, secondly, a general belief in the existence of

* *I.e.* hip-bone.

† *Wild Sports of the World*, J. Greenwood, London, 1862.

various giant races of men seems to be almost as old as humanity. The mythologies of all races and all creeds are full of giants—their existence is one of the most widely spread of all beliefs. To our forefathers, it was an immeasurably more natural thing to discover the bones of a giant than those of an elephant. Sometimes, indeed, one comes across a reputed giant who could scarcely have been an elephant—witness the following:

“A True Report of Hugh Hodson, of Thorneaway, in Cumberland, to S^r. Robert Cewell, of a gyant found at S. Bees, in Cumb’land.

“The said gyant was buried 4 yards deep in the ground, w^{ch} is now a corn feild (*sic*). He was 4 yards and a half long, and was in complete armour: his sword and battle-axe lying by him. His sword was two spans broad, and more than 2 yards long. The head of his battle-axe a yard long, and the shaft of it all of iron, as thick as a man’s thigh, and more than 2 yards long.

“His teeth were 6 inches long, and 2 inches broad; his forehead was more than 2 spans and a half broad. His chine bone could contain 3 pecks of oatmeale. His armour, sword, and battle-axe are at Mr. Sand’s, of Redington, and at Mr. Wyber’s, at St. Bees.”*

It would have simplified matters if only the bones, or the armour, had been found. The bones would naturally have been those of an elephant, while it was not unknown for chiefs in bygone days to have some of their personal property made of exaggerated size, so as to give a false idea of their strength and stature. As this “True Report” stands, one can only conclude that Hugh

* Quoted from Jefferson’s *History and Antiquities of Allerdale above Derwent*, and stated to be taken from the Machel MSS. (Carlisle), vol. vi.

Hodson, of Thornehay, in Cumberland, had the makings of a very expert witness.

The Patagonian Giants.

But the mediæval believers in a giant race were not satisfied by merely finding relics of it, however large. They argued that such a race must still exist; and it so happened that while Magellan was showing the way round the world he incidentally provided the credulous with a legend—that of the gigantic natives of Patagonia—which exercised men's minds for fully three centuries. These may be divided, roughly, into a century of credulity, a century of incredulity, and a century during which it became apparent that the story of the Patagonian giants had a real, if slender, basis of truth. Here is a short outline of the events of these three periods.

The First Period.

The discovery of Patagonia* is due to Magellan, who coasted its eastern shores, and passed through Magellan Strait, which divides it from Tierra del Fuego, in 1520.†

While Magellan's fleet was lying in Port San Julian‡ in June 1520, a gigantic native appeared on the beach near the ships. "This man," says Pigafetta, a companion of Magellan, "was so tall that our heads scarcely came up to his waist, and his voice was like that of a bull." He was well treated, and other natives soon appeared,

* Patagonia, as the geographical name of a region, covers all land between the Rio Negro (in approximately 39° S.) and Magellan Strait; and hence includes the southern portions of Argentina and Chile. The region supposed to be inhabited by giants was in the near vicinity of the Strait.

† Here, and later, I have generally used the epitomes of the early voyages compiled by such writers as Hawkesworth and Burney.

‡ 49½° S., 68° W.—on the Argentine coast. It was here that Magellan strangled Quesada, one of his mutinous captains; and here also that Drake beheaded Thomas Doughty. See Fig. 2.

of whom Herrera,* less hyperbolic than Pigafetta, remarks only that the smallest of them was taller and

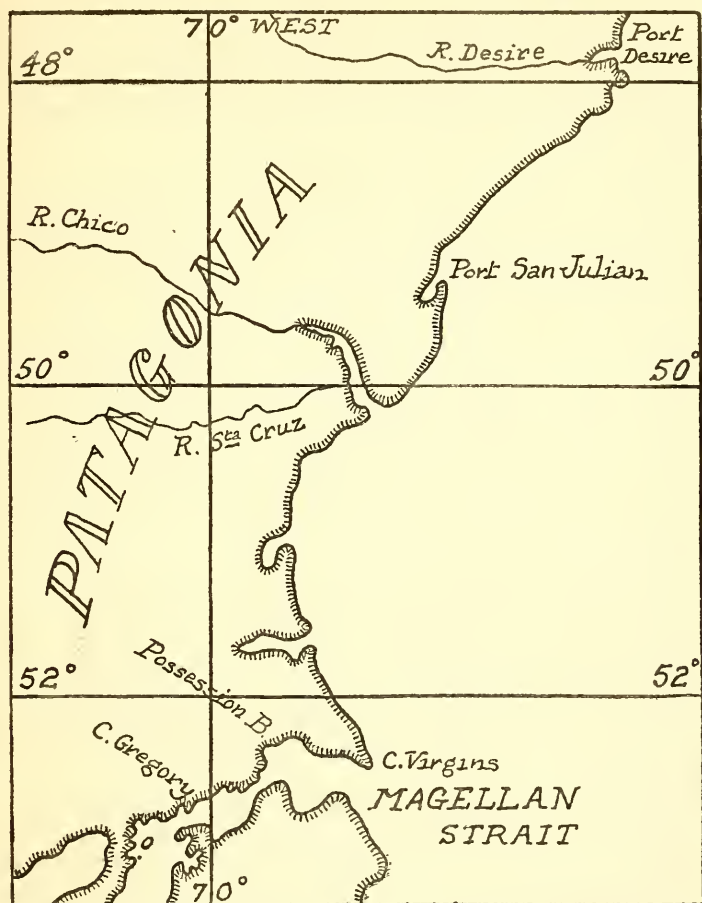


FIG. 2.—Sketch-map, showing various localities in which the “Patagonian Giants” have been encountered.

bulkier than any of the Spaniards.† Magellan gave them the name of “Patagones” (“Pata” being Spanish for

* Official historian of the Spanish voyages to the New World. He did not take part in any.

† According to Max. Transylvanus, the tallest Patagonian seen stood 7 feet 6 inches.

"hoof"), from the guanaco-skin moccasins which they wore. By a stratagem he managed to put two of them in irons,* intending to take them to Europe. Both died before crossing the Line. Pigafetta managed to compile a short glossary of their principal words, in which it is interesting to find the name "Setebos" used (as, later, in *The Tempest*) for their principal deity.

Drake, who anchored in Port San Julian in June 1578, also saw "men of large stature," and had an affray with them, losing two of his crew. They are described as standing a little under 7 feet 6 inches.

Pedro Sarmiento, in February 1580, saw "people of large stature" in Magellan Strait, but their height is not stated; while Tomé Hernandez, who had spent about a year in the Strait in 1584, could say nothing about the stature of the natives, except that they were "very corpulent." As he nearly died of starvation there, he probably felt strongly on the subject.

Anthony Knyvet, who accompanied Cavendish in his disastrous second voyage and traversed the Strait in April 1592, speaks† of having seen Patagonians 14 to 16 spans (i.e. 10½ to 12 feet) in height; and also of having measured several dead bodies of the same size at Port Desire.

Sebald de Weert, who visited the Strait in 1598, speaks of the natives as being 10 or 11 feet high; and Joris Spilbergen, on April 3, 1615, saw "a man of extraordinary tall stature" watching his ships from the south side of the eastern entrance. On an island near by his men found the dead bodies of two natives, half-buried. One was of ordinary stature, but the other 2½ feet taller.

* Two others were, with difficulty, overpowered by nine of the Spaniards and bound; one, however, broke loose, and the other got away later.

† In his *Relation* . . ., for which see Purchas. Burney speaks of it as " . . . a Relation by Anthony Knyvet, which contains many things not credible."

Jacob Le Maire and Wilhelm Schouten are said, but on doubtful authority, to have found skeletons 10 or 11 feet long at Port Desire in December 1615. Aris Clatz, Le Maire's "Commissary" (supercargo), is supposed to have made the discovery, but it is not mentioned in the earliest account of the voyage.*

The Second Period.

It might be thought that, by this time, the existence of men much above the common height in Patagonia was fairly well established. But such was not the case. The voyagers of the late sixteenth century, as a class, seem to have gone out of their way to point out that they had seen no gigantic Patagonians (which was no doubt true); and, as a natural deduction, that none existed (which was unwarrantable). For example, Sir John Narborough, who spent ten months on the Patagonian coast in 1670, expressly denied that the Patagonians were any taller or bigger than other men. He attested that he had often measured the skulls and footprints of the savages whom he met in Magellan Strait, and found these to be of ordinary size; and that the same was the case with the numerous natives whom he had seen at Port St. Julian. It was recalled that John Winter, Drake's disaffected second-in-command, had stated on his return to England that he had seen no giants in the Strait, and that their existence was a fable invented by the Spaniards; and de Gennes, who followed Narborough in 1696, supported his views. On the other hand, Captains Harrington and Carman, commanding two French ships,† saw giants repeatedly in Possession Bay;

* *Journal of the Voyage of Wilhelm Schouten* (Amsterdam, 1617). Clatz's story appeared in the second (1619) edition.

† One appears to have been the *Jaques* of St. Malo.

six on one occasion, seven on another, and yet again "about four hundred men, part of whom were gigantic and part of the common stature." And Frezier was informed by the Spanish authorities at Valdivia, Chile, in 1712, that a tribe of gigantic natives, averaging 9 to 10 feet in height, existed in the interior of southern Patagonia. Oliver van Noort, in 1599, had heard the same story from natives (of the ordinary size) whom he took on board in Magellan Strait and, somewhat barbarously, instructed in the Dutch tongue.

The Third Period.

The visit of Commodore Byron to Magellan Strait in 1764 was long regarded as having definitely established the existence of the Patagonian giants. Even before his time, however, it had come to be recognized that Patagonia was a large place; that its interior was unknown; that it was apparently inhabited by various nomadic tribes differing widely in many ways; and that no inference drawn from the stature of the natives encountered at a particular place could be relied upon to indicate what would be met with elsewhere by the same voyager, or at that place by a later one.

Byron, in the *Dolphin*, anchored inside C. Virgins, at the eastern entrance to the Strait, on December 21, 1764. As he anchored he caught sight of a number of natives on horseback,* waving to invite the strangers on shore. Accordingly, he landed with an armed party, and was at once confronted by a native who appeared to be a chief. The remainder of the Patagonians, some five

* Bulkeley, a survivor from the wreck of the *Wager*, speaks of having seen three mounted Patagonians near C. Virgins on December 12, 1741. This is the earliest recorded instance of their having been seen to use horses.

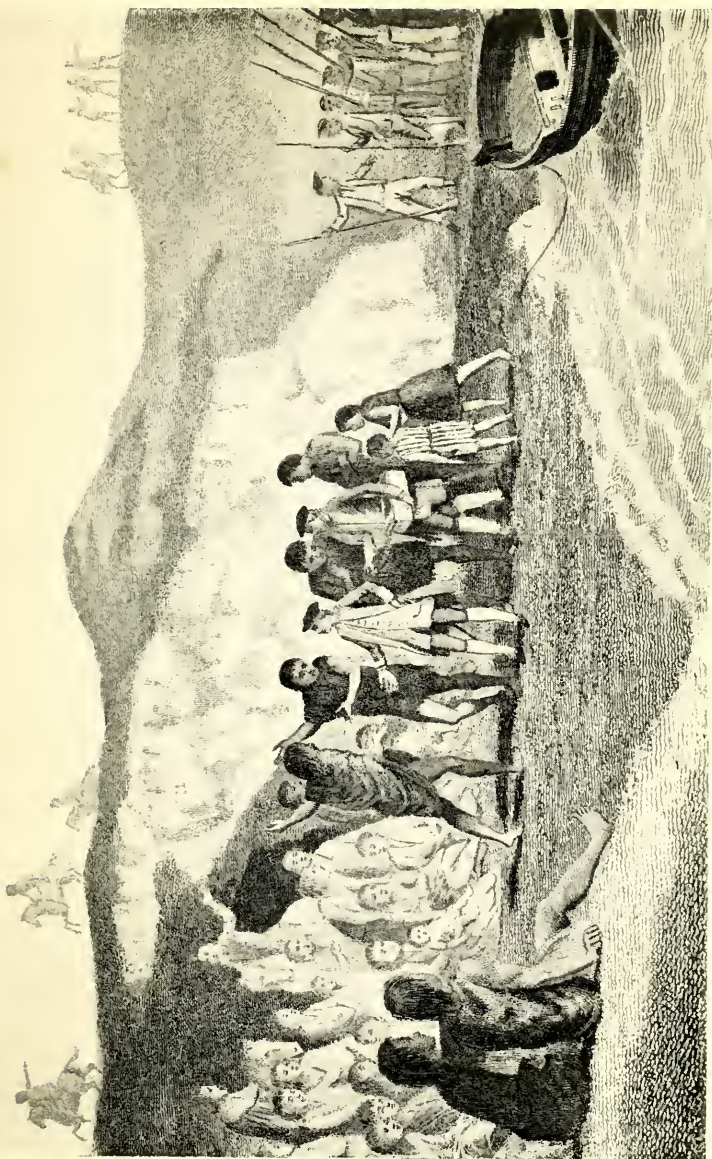


PLATE I

COMMODORE BYRON AND THE GIANTS

*From Hæckesworth's "Voyages of Byron,
Wallis, Carteret, and Cook"*

[Facing page 22]

hundred, kept at a little distance. Here is Byron's (or, rather, Hawkesworth's)* account of the meeting:

"One of them, who afterwards appeared to be a Chief, came towards me: he was of a gigantic stature, and seemed to realize the tales of monsters in a human shape: he had the skin of some wild beast thrown over his shoulders. . . . I did not measure him, but if I may judge of his height by the proportion of his stature to my own, it could not be much less than seven feet. When this frightful Colossus came up, we muttered somewhat to each other as a salutation, and I then walked with him towards his companions. . . ." (See Plate I.)

Later, when friendly relations had been established, he is made to remark:

"Mr. Cumming came up with the tobacco, and I could not but smile at the astonishment which I saw expressed in his countenance, upon perceiving himself, though six feet two inches high, become at once a pigmy among giants; for these people may indeed more properly be called giants than tall men; of the few among us who are full six feet high, scarcely any are broad and muscular in proportion to their stature, but look rather like men of the common bulk, run up accidentally to an unusual height; and a man who should measure only six feet two inches, and equally exceed a stout well-set man of the common stature in breadth and muscle, would strike us rather as being of a gigantic race, than as an individual acci-

* Dr. John Hawkesworth (*ob.* 1773) was paid £6,000 by the Admiralty for editing the journals of Byron, Wallis, Carteret, and Cook (first voyage), which he published in three ponderous volumes just before his death. He was a disciple of Johnson, and in consequence emended the plain English of Byron and the others into Johnsonese, often with absurd results.

dentally anomalous; our sensations, therefore, upon seeing five hundred people, the shortest of whom were at least four inches taller, and bulky in proportion, may be easily imagined."

The *Annual Register* for 1768 contains an account of the same meeting, written by Mid. C. Clerke,* one of Byron's officers. He says:

"... some of them are certainly nine feet, if they do not exceed it. The commodore, who is very near six feet, could but just reach the top of one of their heads, which he attempted, on tip-toe; and there were several taller than him on whom the experiment was tried. They are prodigious stout, and as well and proportionably made as ever I saw people in my life. . . . The women, I think, bear much the same proportion to the men as our Europeans do; there was hardly a man there less than eight feet, most of them considerably more; the women, I believe, run from $7\frac{1}{2}$ to 8."

These remarks show the writer to have been misled by that curious illusion which makes most people overestimate the height of any one who is considerably taller than they are. By experiment, it will be found that the man whose head Commodore Byron could barely touch must have stood just about 8 feet—or, if anything, an inch or so less.†

Wallis and Carteret, who visited C. Virgins two years later (December 1766), took more accurate measurements of the natives (not necessarily, of course, the tribe seen by Byron). Wallis states:

* Afterwards Cook's second-in-command, 1776-1779.

† I stand 6 feet $4\frac{1}{2}$ inches, and span 6 feet 8 inches from finger-tip to finger-tip: and I find that (standing on tip-toe) I can just touch a point, on a vertical wall, 8 feet 6·3 inches from the ground.

“As I had two measuring rods with me, we went round and measured those that appeared to be tallest among them. One of these was six feet seven inches high, several more were six feet five and six feet six inches; but the stature of the greater part of them was from five feet ten to six feet.”

These measurements were in agreement with some obtained a little earlier (May 1766) by Duclos Guyot, one of Bougainville's captains, at a point in the Strait a little westward of C. Virgins. One of his officers measured the shortest of six natives whom he met, and found his height to be a quarter of an inch under 6 feet. He contented himself with noting that “the others were considerably taller.”

Hawkesworth, summing up all the evidence then (1773) available, reached the conclusion that the more settled natives on both sides of the Strait were of ordinary stature, and that the “giants” spent most of their time on the western side of the Andes, and elsewhere in the interior—only visiting the shores of the Strait at rare intervals. He winds up his analysis by remarking triumphantly:

“Upon the whole, it may be reasonably presumed, that the concurrent testimony of late navigators, particularly Commodore Byron, Captain Wallis, and Captain Carteret,* Gentlemen of unquestionable veracity, who are still living, and who not only saw and conversed with these people, but measured them, will put an end to all the doubts that have hitherto been entertained of their existence.”

* It will be noticed that he makes no mention of Cook. The latter, as we might expect, wasted no time in the Strait, but went round the Horn.

But if these "prave 'orts" gladdened the hearts of Lord Monboddo* and other cranks, they were not confirmed by later explorers. No one has yet obtained definite proof that Hawkesworth's supposed giant race actually exists. On the other hand, there is plenty of testimony to the great average stature of many Patagonians. Darwin† says:

"During our previous visit (in January),‡ we had an interview at Cape Gregory with the famous so-called gigantic Patagonians, who gave us a cordial reception. Their height appears greater than it really is, from their large guanaco mantles, their long flowing hair, and general figure: on an average their height is about six feet, with some men taller and only a few shorter; and the women are also tall; altogether they are certainly the tallest race which we anywhere saw."

And Bourne, who spent some time among them *circa* 1849, remarks:

"In person they are large; on first sight they appear absolutely gigantic. They are taller than any other race I have seen, though it is impossible to give any accurate description. The only standard of measurement I had was my own height, which is about five feet ten inches. I could stand very easily under the arms of many of them, and all the men were at least a head taller than myself; their average height I should think is nearly six and a half feet, and there were

* James Burnet, Lord Monboddo (1714-99), was a Scottish lawyer, chiefly remembered by the pertinacity with which he defended his remarkable theory that all children are born with tails—these, in civilized countries, being surreptitiously removed by the midwives. He was also a strenuous advocate for the existence of the Patagonian giants. In justice to the memory of a much-derided man, it should be pointed out that he was far in advance of his time in maintaining that a close relation existed between the physical structure of man and of the higher apes.

† *Voyage of the "Beagle,"* chap. xi.

‡ 1834.

specimens that could have been little less than seven feet high."

Believers in a living race of giants, if they are not satisfied with the Patagonians' average height of 6 feet or a little over (which is considerably above that of any other race) may, if they wish, still believe that the 9-foot men alleged to have been seen by Byron (or, for that matter, Knyvet's men of 10 to 12 feet) were stray members of a tribe of colossi still surviving in the heart of Patagonia. It cannot definitely be said that such is an impossibility. There are enormous areas in southern Patagonia which are still quite unexplored. It may be remembered that a considerable sensation was caused in 1897-8 by the discovery, in a cave at Consuelo Cove, Last Hope Inlet, on the western coast of Patagonia, of what was, apparently, some quite fresh skin from a *Mylodon*, or giant sloth—an animal hitherto supposed to have been extinct since prehistoric times. An expedition,* sent out to test the supposition that surviving specimens of the *Mylodon* might still be found in some remote and unknown regions of Patagonia, left the question undecided; but it showed conclusively that very much more exploration would have to be effected before the theory could be rejected as impossible.

If we do not wish to locate our giant race in South America, we can fall back on the recent reports of the mysterious "Migues" of the Himalayan slopes; who are stated (by their native neighbours) to be from 10 to 12 feet high.† They have never been seen by a

* Led by the late H. Hesketh Prichard, and financed by the late Sir C. A. Pearson. It did not succeed in reaching Last Hope Inlet. See *Through the Heart of Patagonia*, by H. Hesketh Prichard (London, 1902).

† They are also said to be white, hairy, and extremely fond of honey—points which naturally suggest that they are really snow-bears. It must be remembered, however, that bears are quite as familiar a sight to the natives as omnibuses to the Londoner.

white man, although the Mount Everest expeditions heard some vague rumours about them.

But we need not go far to find an actual race of giants—although they are an artificial product. They are to be found where one would scarcely think of looking for them—in Japan. The average Japanese wrestler is of huge size, sometimes exceeding 7 feet, and of more than corresponding bulk. There is a delightful description of them, and of the incredibly funny ceremonies attending their contests, in the late Lord Curzon's *Tales of Travel*—a book which incidentally reveals its author as possessed of a very keen sense of humour. He compares these vast beings, very justly, to Daniel Lambert*—and speaks of their evolution thus:

“I afterwards inquired how it was that this strange and abnormal type of manhood was produced, and I learned that it was by the practice of eugenics *in excelsis*. The wrestlers are selected in boyhood from parents of unusual size; they are dieted and treated from the earliest years; as they grow up and enter the ring they are attended by a special bodyguard of masseurs, trainers, barbers, clothiers, and cooks; they are encouraged to consume an incredible amount of strength-producing food; and they constitute a separate guild, graded, numbered, and registered according to their capacity.”

There is no doubt that careful breeding can accomplish wonders—for example, I believe that the mastiff and the pug (both, of them, alas, breeds which have been ousted from favour by the Chow, the much-maligned Alsatian, and the Pekingese) have been developed from a

* One of the fattest men who ever lived (1770–1809). He weighed 739 lbs.—52 stone 11 lbs.

common strain—but it is difficult of application to human beings. Still, we may yet see giants bred in small numbers for show purposes—in which case Bishop Berkeley's (alleged) artificial giant* will no longer be unique among white men.

But I question whether there is, or ever has been, a race of men averaging, say, 8 feet or more in height. It is true that isolated cases of greater stature have occurred, but they are the result of accidental disease, and not of heredity. The giant of the dime-show and the music-hall is of interest to medical men with an eye to the dissection of his pituitary gland, and in their sight he may be a desirable specimen. But he is a poor specimen of humanity; disproportionate,† feeble, ailing, and with acromegalic hands, jaw, and feet. Machnov (9 feet 3 inches), who possessed all these stigmata in a marked degree, could never have been taken for a normal man, even in a photograph which gave no indication of scale. Such giants die young;‡ and if they beget children (which is seldom), these, more fortunate, are usually of normal size.§

* It used to be related that the great and good George Berkeley, Bishop of Cloyne, better known for his idealistic philosophy and his unequalled power of writing dialogue, had succeeded, by a system of dietary (on the lines of Wells' "Food of the Gods"), in making an unfortunate child grow into a giant. The true facts are these. A peasant boy named Cornelius Magrath, suffering from gigantism and half-starved for lack of food, was brought to the notice of Berkeley, who nursed him back to comparative health in his own house, and afterwards befriended him in many ways. Magrath died in 1760, aged 24. His skeleton (7 feet 8 inches) is in Trinity College, Dublin.

† Chang Wow Gow (1845–1893) is, I believe, the only case on record of a man 8 feet high and perfectly proportioned. He was a splendid specimen of humanity and extremely intelligent, speaking six languages. At the Prince of Wales' request, he once wrote his name on a wall at the height of over 10 feet from the floor.

‡ In almost every case, before thirty. Chang died at forty-eight; M. C. Miller (7 feet 10 inches) at sixty. I believe that an American giant, Captain M. V. Bates (who stood about 7 feet 10 inches and his wife 7 feet 9 inches), lived to be over fifty; but I am not certain of this.

§ With regard to the children of giants, it should be noted that J. R. Forster, who sailed (and squabbled) with Cook on his second voyage round the world, records that the average height of the citizen of Potsdam was unusually great,

As Domain, maker of robots, remarks:* "Our planet is too small for giants." There is a structural limit to the useful size of all creatures built of flesh and bone. A man's weight varies, roughly, as the cube of his height; but his strength (which depends on the cross-section of his muscles) as the square of it. If, say, a man of 5 feet 10 inches were to be increased by one-seventh all round, he would become about half as heavy again, but he would only be stronger in the proportion of 64 to 49, so that he would actually be less efficient, physically, than he was before—and, both now and always, the future is to the efficient. The huge beasts and birds of prehistoric times, and some which survived till a quite recent day, died out because, even if Man spared them, Nature would not. And so, even if it be true that "there were giants in the earth in those days,"† it is probable that their descendants have long since joined the Dodo, the Mammoth, the Moa, and Steller's Sea-cow in whatever limbo is reserved for Nature's discarded experiments.

a fact which he ascribes to their frequent intermarriages with the soldiers of the King of Prussia's "giant regiment."

* In Kapek's *R.U.R.*—a strange play which has given a new word to the English language.

† Genesis vi. 4.

THREE STRANGE SOUNDS

THE CRY OF MEMNON

*Still from his chair of porphyry gaunt Memnon strains his lidless
eyes*

Across the empty land, and cries each yellow morning unto thee.

IF Tennyson, as seems likely, excelled Wilde as a poet, he nevertheless showed himself inferior to the author of *The Sphinx** in accuracy when he wrote:

. . . from her lips, as morn from Memnon, drew
Rivers of melodies.

The sounds which are recorded as having been emitted by the famous "vocal statue of Memnon" were neither many nor melodious. Infrequently, but always at sunrise, those who stood near it long ago might hear a thin, strident sound, like the breaking of a harp string. That was all—an aimless cry heard at rare intervals during a relatively short period of two hundred years, a period preceded and followed by many centuries of silence. Yet it was a phenomenon of which hardly any similar case is on record—and it was not, it should seem, a deception. The statue, which had been silent for so long, and has again sunk into silence, did once acquire and exercise some strange inherent power of saluting the sun.

The statue of Memnon—which, incidentally, is not

* The initial quotation is taken from that bizarre poem—one of the few really original pieces that Wilde ever wrote. It will be noticed that the metre, though not the form, is that of *In Memoriam*.

the statue of Memnon, but of Amenophis III or Amenhotep—was erected about 1500 B.C. Its architect (we can hardly, *pace* Mr. Epstein, call him a sculptor) was, rather confusingly, also named Amenhotep, son of Hapu. It forms one member of a pair of twin colossi, still standing, about a mile from the western bank of the Nile, among the ruins of Thebes.*

Here is their creator's own description of them:†

“For my Lord the King was created the monument of sandstone. Thus did I, . . . causing to be made two images of a most noble hard stone, in his likeness . . . wonderful for their breadth, lofty in their height, the stature whereof made the gate-tower to look small. Forty cubits was their measure. In the glorious sandstone mountain‡ wrought I them, on this side and on that, on the east side and on the west. Furthermore, I caused to be built eight ships, whereon they were carried up. . . .”

Even in their present mutilated and defaced condition, the two colossi are a most impressive sight. Each is some 50 feet high,§ and there is about the same distance between them. They sit side by side, looking S.S.E. towards the Nile.

The western figure, a single piece of stone, is featureless, and the breast, legs, and feet are badly damaged. The eastern, which is the celebrated vocal statue, has obviously suffered far more extensive mutilation. From the pedestal

* See Frontispiece.

† From an inscription on his own statue, now in the Boulak Museum, at Cairo.

‡ Obviously, from the context, some quarry down river—possibly that of Toora, near Cairo.

§ Wilkinson and Curzon, both writing from personal observation, differ in the heights which they assign. Wilkinson made the western statue, by sextant angles, 47 feet high, and the eastern (the vocal statue) 47 feet 9 inches, by actual measurement. Curzon gives 51 feet for each. Both sets of dimensions are exclusive of the pedestal.

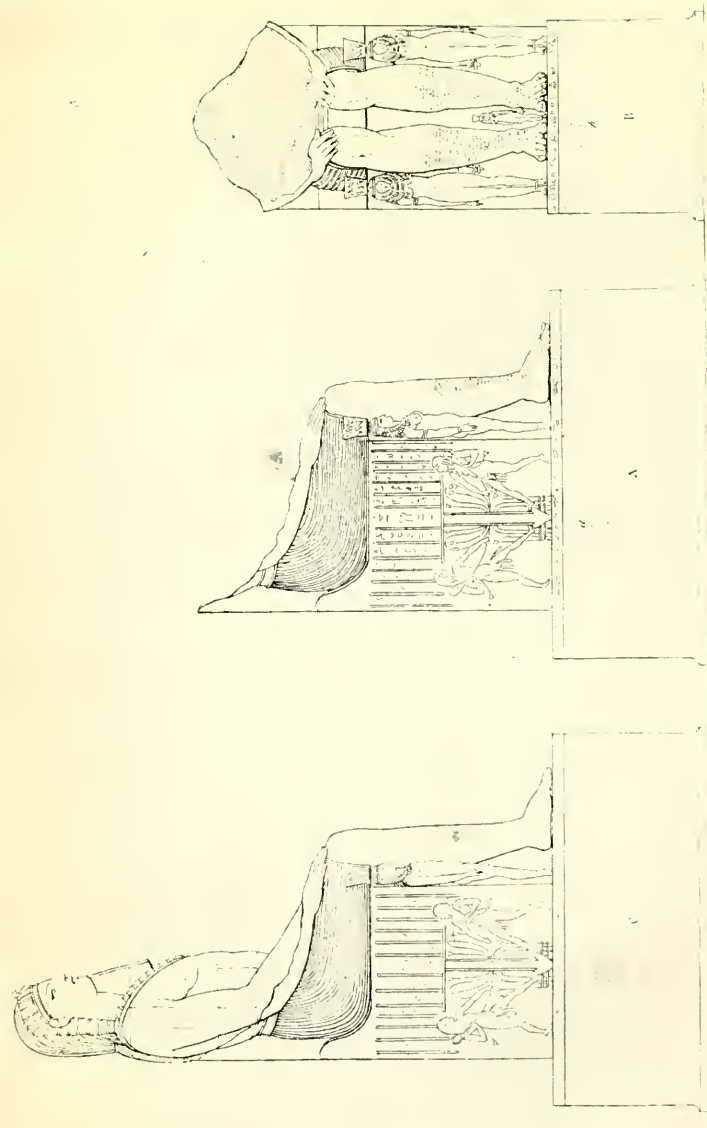


PLATE II

MEMNON, BEFORE AND AFTER RECONSTRUCTION

NOTE.—These drawings are hypothetical, and not absolutely accurate in detail. The inscriptions on the legs of the statue will be noticed

*From Letronne's "La Statue
Vocale de Memnon"*

(Facing page 32)

to the waist it is a single block, extensively cracked; from the waist upwards it is composed of five tiers of lighter stone, as if foreshadowing that puerile monstrosity, the 100-foot "statue of Columbus" recently constructed at Palos. As with its fellow, the breast is damaged and the features unrecognizable.

The material of the western statue, and of the lower part of the eastern, is stated* to be a "coarse hard gritstone." The upper part of the eastern statue is of sandstone.

As already remarked, the eastern or reconstructed statue is that which is credited with having once emitted sounds. Before discussing this question, however, it may be as well to subjoin a short outline of its history (so far as that is known or conjectured)—a personal selection from the opinions expressed by several experts, often at variance.† The dates are approximate.

- | | |
|-----------|---|
| 1500 B.C. | Erection of the statues. |
| 524 B.C. | The statues defaced by Cambyses.‡ |
| 27 B.C. | Upper half of the eastern statue thrown down by an earthquake, which also caused additional damage to the western statue. |
| 20 B.C. | Strabo, the historian, visited Thebes, and recorded the fact that, both by report and his own observation, the eastern statue emitted a sound at sunrise. |

* Wilkinson.

† The literature of the subject is quite extensive. In the short sketch here given I have relied chiefly upon the following:

La Statue Vocale de Memnon. J. A. Letronne (Paris, 1833).

Modern Egypt and Thebes. Sir G. Wilkinson (London, 1843).

The Voice of Memnon. Lord Curzon of Kedleston. This originally appeared in *The Edinburgh Review* for July 1886, and was reprinted, with slight alterations, in Curzon's *Tales of Travel* (London, 1923).

‡ Cambyses, King of Persia, subjugated Egypt about 525 B.C., and ruled it for some five years, during which time he is credited with having done an almost incredible amount of wanton damage to its temples and monuments. Possibly, like Cromwell, he may have been a dog with a bad name.

19 A.D.	The sound heard by Germanicus.
90 A.D.	The sound heard by Juvenal.
130 A.D.	The sound heard by the Emperor Hadrian, and by Pausanias.
196 A.D.	Last recorded occasion on which the sound was heard.
(later)	Eastern statue reconstructed.

The dates on which the sound was heard, as given above, are only intended to provide a few points of reference. During the period 20 B.C.—A.D. 196 it seems to have occurred quite frequently, if irregularly—certainly not every morning, but probably several times a year at least. The authorities as to the simple fact of it being heard include Strabo, Pliny (not from personal observation), Pausanias, and Juvenal; the last of whom speaks of “*dimidio Memnone*”—*halved* Memnon, indicating that the statue had not then been restored. But, in addition to these, there is a considerable body of testimony inscribed on the statue itself. Its feet exhibit a series of eighty-seven inscriptions* in Greek and Latin (there are none in Egyptian characters) made by visitors who, like modern trippers, were moved by the desire to carve their name on something, and who took the opportunity of recording that they had, or had not, heard the cry of Memnon. These inscriptions range over a period of about two centuries. The last was made in A.D. 196.

As to the nature of the sound, Strabo speaks of it as resembling the sound of a slight blow. Pausanias states that it can only be compared to the sound made when the string of a lyre breaks. One of the inscriptions describes it as a high-pitched note, like that produced by striking brass.

The inscriptions contain some thirty-nine references to

* These were all copied by Letronne, and are illustrated in his monograph.

the time of day at which the sound was heard. Tabulated, the results are as follows:

Before sunrise	2
At sunrise	18
Less than an hour after	8
An hour after	6
Less than two hours after	2
Two hours after	3

The months most frequently mentioned are February and March, probably because they were those in which visitors to the statue could most easily ascend the Nile. Several of these heard the sound more than once—on the other hand, two inscriptions record success only at the second visit; and one, not until the third.

It is not absolutely certain, but almost so, that the statue has been silent since about A.D. 200 or so. There is one recorded instance of its having sounded in quite modern times; but this event is attested in so curiously roundabout a fashion that its value as evidence is exceedingly slight. Here is the original authority, *verbatim*. (*Revue Encyclopédique*, Tom. IX, p. 598. Paris, March 1821.)

“EGYPTE.—*Antiquité*.—*Statue de Memnon*.—Dans une lettre adressée à l’ambassadeur de Russie à la cour de Rome, sir A. Smith, voyageur anglais, qui est maintenant à Thèbes, dit avoir examiné lui-même, avec sa nombreuse escorte, la célèbre statue de Memnon. A six heures du matin, il a entendu très-distinctement les sons dont il a été parlé si souvent dans l’antiquité. *Memnonis saxea effigies, ubi radiis solis icta est, vocalem sonum reddens*. Tac. ann. 2. M. Smith assure que ce bruit mystérieux ne sortait pas de la statue, qui a été renversée par un tremblement de terre, mais du

piédestal; il le regarde comme le résultat de la percussion de l'air sur les pierres de ce piédestal, qui, selon lui, sont disposées de manière à produire cet effet."

It is somewhat singular, but apparently true, that the Theban investigations of this distinctively named English traveller, "sir A. Smith," would have been lost to posterity had it not been for this short abstract, in a French periodical, of a letter written by him to a Russian friend resident in Italy. During the past century, Smith's visit has often been cited as proving that the cry of Memnon has been heard in modern times, but no one seems to have discovered any better authority for it than the *Revue Encyclopédique*. For my own part, although I have not been able to spare the ten years or so which would be required to make a really exhaustive search, I have done my limited best to throw more light on the subject—without success. I have not succeeded either in identifying "sir A. Smith" or in finding any further account of his travels in Egypt.

It will be noticed that he is stated to have assured himself that the sound came from the pedestal—which is a squared socle, some 13 feet high,* and in one with the lower half of the statue itself. It is curious that this observation, reported to have been made by the last man who heard the cry, is in close agreement with the account given by Strabo, who was the first. Speaking from his own experience (he visited Memnon in 20 B.C., in company with Aelius Gallus, the Roman governor of Egypt), he relates that he heard a sound, but "could not affirm whether it proceeded from the pedestal or from the statue itself, or even from some of those who stood near

* About 6 feet of this are above the sand and 7 below.

its base.”* His last suggestion—that the whole thing was a trick—is, in itself, plausible; and it is quite possible that the sound heard (if it was heard) by Smith really emanated from one of his “numerous escort,” anxious that he should not be disappointed; but, as will be seen, this hypothesis does not cover the events of the period during which the sound was of frequent occurrence. Wilkinson, who was firmly convinced that the sound was an imposture, makes no mention of Smith’s visit at all, although writing twenty years after it had become generally known: he merely remarks, in a footnote, “More than one traveller has repaired to the statue before sunrise in hopes of hearing the sound,” leaving it to be inferred that none succeeded in doing so.

But, whatever conclusion may be formed as to the reality of the events associated with the mysterious Smith, there can be little doubt that the cry of Memnon was repeatedly heard, by quite a cloud of witnesses, during the first two centuries of the Christian era. I turn to the question of how that sound was produced. Opinion on this subject is as sharply and naturally divided as the original half of the statue itself; in which a fissure, running east and west, extends from the waist to the pedestal. Some hold that the sound was the result of human agency: in plainer language, a pious fraud—others regard it as a rare, but not unknown, phenomenon due to natural causes.

The hypothesis of fraud has, at first sight, a great deal to recommend it. The suggestion of imposture is inevitable. Here is a statue, of a semi-religious character, visited by crowds of superstitious and ignorant persons who have been drawn to it by the belief that it has miraculous powers. Where miracles are expected, miracles

* Strabo, lib. xvii.

can usually be made to occur—witness the punctual and obliging* manner in which the blood of St. Januarius usually liquefies several times a year in Naples cathedral.

So far so good. But three questions remain to be answered, namely:

1. How was it done?
2. Who did it?
3. Why was it done?

To the first question there are several possible replies, some of which are more plausible than others. The sound may have come from some interested person who had stationed himself (possibly, like the more speculative type of bookmaker, with a few “minders” in attendance) near the statue—and it may have been produced by a small pair of cymbals, or an actual lyre, or some device of the kind; which, as any Customs searcher will testify, could easily be concealed in the thaumaturgist’s clothing. Such is the simplest explanation. On the other hand, some have imagined that the sound was produced in the interior of the figure. Two French writers, Langlès and Salverte, worked out the details of an elaborate mechanism which, according to them, must have filled quite a large space in Memnon’s interior.† Unfortunately, no trace of such mechanism or cavity remains.

A statement published by Sir Gardner Wilkinson, F.R.S., in 1843, was long regarded as having demonstrated not only the fact of fraud, but also the way in which this was effected. Here it is:‡

* So much so that it found no difficulty in conforming with the alteration of the calendar effected by Pope Gregory XIII in 1582.

† See Salverte’s *Des Sciences occultes* . . . (Paris, 1829). He provided for a system of hammers, striking on sonorous stones, and driven by a water-clock. The machinery was let off by the expansion of a metal rod, on which the sun’s rays were concentrated by a lens which the figure held between its lips. He does not seem to have thought of adding an aerial and a few loud-speakers.

‡ *Modern Egypt and Thebes*, vol. ii, p. 161.

“In the lap of the statue is a stone, which, on being struck, emits a metallic sound, that might still be made use of to deceive a visitor who was predisposed to believe its powers; and from its position, and the squared space cut in the block behind, as if to admit a person who might thus lie concealed from the most scrupulous observer in the plain below, it seems to have been used after the restoration of the statue; and another similar recess exists beneath the present site of this stone, which might have been intended for the same purpose when the statue was in its mutilated condition.”

As Curzon has pointed out, Wilkinson is at some pains to make his theory of imposture absolutely unassailable. He is not content with finding one suspicious recess; he produces two, one used when only the lower half of the statue remained, the other after the reconstruction. Furthermore, he “proceeds to narrate” that while he had considered, in 1824, that the “metallic sound” produced by striking his stone did not tally with the traditional sound of Memnon, he subsequently (in 1830) came across the inscription* which compared this to the sound made by striking brass. Fortified by this, he stationed some fellahin below, and tapped his “sonorous block” with a small hammer.† His audience, on being asked what they heard, replied, “*Ente betidrob e’nahás*”: which is, being interpreted, “You are striking brass.”

Unfortunately for Wilkinson’s reputation, Curzon (who visited the statue in 1886) discovered, with the help of a ladder and a foot-rule, that his “sonorous block” was simply a stone which had fallen from the

* On the statue itself, by one Ballilla. See p. 34.

† As Wilkinson recalls, Strabo declared the sound to resemble a slight blow.

superstructure (which was not, apparently, in place when the sounds used to be heard) and had jammed in the natural fissure dividing the lower (original) half of the figure. Nor was this all. Wilkinson's "squared space" was simply the gap which was left in the superstructure when the stone fell, and his other "similar recess" was nothing but the fissure itself! Wilkinson had obviously distorted the evidence of his own eyes into conformity with his prejudices.

So far as is known, there is nothing in the structure of the figure which affords any positive evidence of fraud. On the other hand, it is quite possible that the sounds could have been made by someone standing near it.

The remaining questions—"who did it" and "why was it done"—can best be discussed jointly, for each involves the other. The obvious answer to them is "The Egyptian priests, to gain prestige and/or money."

Unfortunately, this is contradicted by the facts. There is nothing which would lead us to suppose that the Egyptian priests were any more averse to "working the oracle" than priests of all creeds, ages, and nations have generally shown themselves to be. But how comes it that they should have so shamefully neglected their opportunities? During many centuries the statue stood, intact, in front of the splendid temple of King Amenhotep. There is no contemporary inscription—no vestige of evidence at all—that during this period, when the priests and their strange religion held full sway, the cry of Memnon was ever heard.* As was first pointed out by Letronne, there are no Egyptian inscriptions on the statue, and nothing to show that it was ever regarded

* Juvenal, it is true, is responsible for the statement that "when mutilated by Cambyzes, the statue which saluted both the sun and the king afterwards saluted only the sun." But such a statement, made hundreds of years after the event, is not evidence.

by the Egyptians as miraculous. This was reserved for its Greek visitors, fifteen centuries after its erection, when Egypt was in Roman hands. The Greeks probably knew the statue as Memnon, because, if it sounded at sunrise, they would naturally associate it with the fabled son of the Dawn; but to the Egyptians it was merely Amenhotep—and, it would seem, not an object of worship at all.*

Even if it be supposed that the priests, or any other interested persons (finding that the statue was, or might be, worshipped as Memnon by alien visitors) decided to make it appear to be vocal, one would at least imagine that there would have been some method in their proceedings. One would, for example, have expected that the sound would be heard when, and as often as, best suited the convenience of its visitors. In particular, one would imagine that if it were visited by any exalted person, whom it would be impolitic to offend, the sound which that person had come to hear would most undoubtedly be heard. But the facts are quite otherwise. If we are to judge by the inscriptions, many visitors never heard the cry of Memnon at all; many others must have had to make repeated pilgrimages before they heard it; and, while it is true that the statue apparently performed three times† for the Emperor Hadrian, it is equally true that it remained obstinately silent on the first occasion when his consort paid it a visit (a fact which, as is duly recorded on the statue by one of her ladies-in-waiting, enraged her exceedingly)—while the Emperor Septimius

* Much ink has been spilt on the question of how the mythical Greek hero Memnon came to be associated with various statues and temples in Egypt. Wilkinson has pointed out that *Miamun* was a title of Rameses II. Either this name or Amenophis might easily have been converted into *Memnon* by credulous Greek immigrants, eager to find support for the Homeric legends.

† On separate days. One or two of the inscriptions, however, record that the sound was heard twice on the same morning.

Severus never succeeded in hearing the sound at all. It is past all believing that those responsible for the supposed deception could have acted with such wanton disregard of the feelings of the great. Such behaviour presupposes an absolute lack of elementary common sense—it would have been that of fools, not knaves.

There is much more to be said for the opposition theory; indeed, except for the sake of completeness, such a proceeding savours of “blacking the chimney,” for this theory is now generally accepted. On the supposition that the sound was a purely natural phenomenon, many of the difficulties connected with it—e.g. the varying descriptions of the sound itself and of the time of day when it was heard—become much less formidable. Such a phenomenon would almost certainly be irregular both in its nature and its recurrence.

The sound was only heard at or near sunrise; it varied in note and intensity; and it occurred, at irregular intervals, during a period which apparently began when the upper half of the statue was demolished (and the lower half split) and ended as soon as (but, possibly, before) the upper half was rebuilt. From these data it is difficult to avoid drawing the conclusion that the sound was caused by the sun’s rays warming the cleft and truncated lower half of the statue; that it was produced by the unequal expansion of the two portions of this fractured monolith—causing them to move, fractionally, one against the other; and that they no longer had free scope for this interplay when they were compelled to support the great weight of the rebuilt upper portion.*

Although no exactly similar case is known, there is a certain amount of evidence which goes to show that in all probability the cry of Memnon was due to a natural

* See Plate II.

cause. Jomard, Jollois, and Devilliers, three of the scientists who accompanied Napoleon's expedition to Egypt, heard a similar sound, at sunrise, on two occasions—once in the granite quarries at Syene and once in a temple at Karnak. They describe it (exactly in the manner of Pausanias) as resembling the sound made by a breaking string. In their opinion it was caused by air, occluded in the crevices of the stone and expanded by heat, making its way out. Rather singularly, they did not connect their experience with the sounds traditionally accredited to Memnon. These they regarded as fabulous. Their experience at Karnak was confirmed later by Brugsch, who was there in 1851 and also heard the sound in question.

To my mind, the theory of unequal expansion has more to recommend it than that of occluded air, although the latter was also put forward independently by Humboldt as explaining certain sounds, like the note of an organ, sometimes heard near sunrise at various points on the Orinoco.* If it be the true explanation, it is difficult to understand why such sounds should not be fairly common wherever stone of a suitable kind is exposed to changes of temperature. On such an assumption, for example, most of the other Egyptian statues and buildings should also emit sounds.

On the theory of unequal expansion, it is true, we can only escape from this objection by supposing that the sounds heard at Syene and Karnak came from stones which, like the statue, had also been fractured in a somewhat similar manner. There is no evidence for or against this supposition.

In any event, it seems most likely that the cry of Memnon had its origin in the fissure dividing the trun-

* Humboldt did not hear these sounds himself.

cated monolith, and that if the circumstances of the fracture had been slightly different the sound probably would never have been heard at all; just as it ceased when the self-adjustment of the two portions was disturbed by the superimposed load.* It must be remembered, however, that there is no actual evidence that the reconstruction of the upper portion synchronized with Memnon's renewed silence, since the date of the reconstruction is not known, although tradition associates it with the Emperor Septimius Severus (in whose reign the sound ceased).

Not only is the date of the reconstruction obscure,† but also its motive. It was possibly done with a view to propitiating Memnon, in which case it may well have been ordered by Severus (who, as already stated, paid the statue a fruitless visit), particularly since such a piece of work demanded resources of labour and material which only an Emperor could command. The tiers of the Roman work are an eloquent tribute to the engineering ability of Amenhotep, son of Hapu, and his men. In its original monolithic condition, the complete statue must have weighed some 1,200 tons; yet the Egyptians hewed it in the "glorious sandstone mountain," transported it many miles by water, and successfully erected it—all with the crudest of appliances. I do not imagine that many modern engineers would care to prepare an estimate for such a job, and it is no disgrace to the Romans that a similar feat proved beyond their powers. To reconstruct the figure even as it now stands was a very considerable achievement.

A minor point of interest in connection with the

* I regard the "sir A. Smith" incident as "not proven."

† The left foot was certainly repaired after the date of Hadrian's visit (c. A.D. 130), for one of the cramps has been let in through the inscription recording this.

statue of Memnon is the fate of the missing upper half. It does not seem to be anywhere in the vicinity of the figure; and, weighing some 500 tons, it was scarcely likely to have been removed in one piece. On the other hand, if it were broken in the fall, or subsequently,* the pieces would seem to have been removed (for some unknown motive) before the reconstruction, or else deliberately rejected—for the new work is of distinctly lighter stone. One is almost tempted to believe that the Roman engineers found it, still whole, near by; and that, unable to replace it, they found means to break it up and remove it, lest it put them to open shame.

PARRY'S CANNON

The cry of Memnon is an example of a strange sound which is known, by name at least, to most people. On the other hand, the remarkable incident which occurred in the course of some experiments on the velocity of sound made by Captain W. E. Parry in 1822 seems to have been almost forgotten.

Parry, who had already made himself famous by reaching Melville Island, half-way to Bering Strait, while searching for the N.W. Passage in 1819,† spent the winter of 1821–22 at Winter Island (66° N., 83° W.). He was not the man to waste his time; and during this period of enforced inactivity he carried out, with the assistance of his officers, a very considerable amount of scientific research. The results fill a quarto volume of

* If it were deliberately broken up, this must have been an exceedingly difficult task. Wilkinson, however, speaks of a statue in the palace of Rameses II which he took to be Shelley's Ozymandias, and which was so utterly shattered that it almost seemed to have been blown up.

† By so doing he secured the Parliamentary reward of £5,000 offered in 1818 to the first explorer who, sailing westward to the north of the American continent, should pass the meridian of Long. 110° W.

432 pages;* and a notion of the diligence with which the work was done may be gathered from the fact that his astronomer, the Rev. George Fisher, took no less than 2,500 "lunar distances" (for determining longitude) in December 1821,† and an equal number in the following March.

Amongst his other researches, Parry made a series of experiments to determine whether the normal velocity of sound underwent any noticeable alteration at low temperatures. His opportunities for making them were excellent. The temperatures available ranged from about -45° to $+40^{\circ}$ Fahr., and he was able to use an absolutely ideal testing-ground. Between Winter Island and the mainland of Melville Peninsula, to the south-westward, is an arm of the sea—practically land-locked and (during the winter) frozen hard. Parry named it the Frozen Sea. Here, on eighteen days between December 29, 1821, and June 18, 1822, Parry made his experiments, from which he deduced the expected result that the average velocity of sound decreased slightly as the temperature fell, the amount of alteration being small and uniform. But on one occasion he encountered another deviation from the normal which, it is safe to say, he did not at all expect.

The experiments of February 9, 1822, were made as follows. A base-line slightly over a mile (5,645 feet) long was carefully marked out on the surface of the ice. At one end Parry mounted a six-pounder gun and its crew, with an officer in charge. Together with another observer (Fisher), he stationed himself at the opposite end of the base. The gun was given an elevation of

* *Appendix to Captain Parry's Journal of a Second Voyage*, London, 1825.

† It is sad to record that the mean of these, although they were all taken on shore, differed from that of the March observations by no less than 14'.

about 10° , and pointed towards the observers. It is hardly necessary to add that it fired a blank charge. The temperature was -25° Fahr.

The procedure was as follows. The officer at the firing-point, when assured that the observers were ready, gave the order to fire the gun. Each observer noted independently, by a pocket chronometer,* the interval between seeing the flash of the gun and hearing the report. It should be added that on this and every occasion the experiments were made late at night, in order to avoid atmospheric disturbances. Both flash and report, therefore, could be noted with very fair accuracy.

Fifteen rounds were fired. The observed times agreed well, and gave a mean velocity of 1,023 feet per second. But Parry and Fisher were perplexed to notice that, on several occasions, the order "Fire," which was plainly audible (although uttered something over a mile away) reached them about half a second *after* the report of the gun. Here is Fisher's own statement, extracted from his official "Abstract of Experiments to determine the Velocity of Sound" (*loc. cit.*, p. 239).

"The Experiments on the 9th February, 1822, were attended with a singular circumstance, which was—the officer's word of command 'Fire' was several times distinctly heard both by Captain Parry and myself about one beat of the chronometer† *after* the report of the gun; from which it would appear, that the velocity of sound depends in some measure upon

* The pocket chronometer—a watch with the spring-detent or chronometer escapement—is now obsolete, on account of its liability to be stopped in the pocket by a shake or jar. Unlike a lever watch, it will not restart itself. Stop-watches were not in common use until long after Parry's time.

† This beat eight times in three seconds; an unusual and inconvenient arrangement. Most pocket chronometers have an "18,000 train," and beat five times in two seconds.

its intensity. The word 'fire' was never heard during any of the other experiments; upon this occasion the night was calm and clear, the thermometer 25° below zero, the barometer 28.84 inches, which was lower than it had ever been observed before at Winter Island. Upon comparing the intervals between the flash and report of a musket with the gun, upon other occasions, there appears to be no assignable difference."

The exact meaning of the last sentence eludes me; it is not very clearly expressed. Fisher leaves the reader in doubt as to whether, on this particular occasion, comparative tests between gun and musket did show a difference; or whether no such tests were made on that occasion, but that they were made at some later date, and no difference observed. I assume that, whenever the tests may have been made, the object of them was to test Fisher's theory that the velocity of the sound depended on its intensity. That they gave entirely negative results is not altogether surprising; so far as I know, the phenomenon of "Parry's cannon" remains unique, not only in his experience but universally.

It is easy to be wise after the event. In the *Philosophical Transactions* for 1860 there appeared a learned paper on the theory of sound,* written by the Rev. S. Earnshaw. In this he lays down and elaborates the theory that, in certain circumstances, waves of sound have a power of propagating themselves, in advance, along the path which they are travelling. He continues:

"I should expect, therefore, that in circumstances where the human voice can be heard at a sufficiently long distance, the command to fire a gun, if instantly obeyed, and the *report* of the gun might be heard at

* "On the Mathematical Theory of Sound" (*loc. cit.*, pp. 133-48).

a long distance in an inverse order; i.e. *first* the report of the gun, and *then* the word 'fire.'"

(He next proceeds to lay a pitfall for the inquiring reader by referring, in a footnote, to Parry's singular experience, and citing the wrong authority for it.)*

This is "all merry capital," but his theory does not square with the facts. The circumstances which he postulates, allowing the human voice to be heard at long distances, are not at all unusual in the Arctic. During Parry's third voyage, for example, Lieutenant Foster, one of his officers, found that it was perfectly possible to carry on a conversation across the frozen surface of Port Bowen Harbour† between the ship and a shore observatory, distant no less than a mile and a quarter. Similar acoustic conditions prevailed during the period of Parry's experiments on the Frozen Sea. Yet on no other occasion did the sound of the gun outrun the order to fire it—nor, as will be noticed, did this invariably occur even during the series made on February 9, 1822.

Sir G. B. Airy, in his treatise on Sound,‡ suggested that the cause of the anomaly was physiological; that owing to the abnormal cold the observer's power of apprehending sound was affected, and less sensitive for some noises than for others. He suggests, in fact, a sort of delay-action between ear and brain, which affected the order more than the report. But this theory, also, is open to the same objection. The cold was not so great as that experienced in several of the other experiments; for example, those of February 16th (a week later) were

* He remarks: "See Suppt. to Appendix of *Parry's Voyage in 1819-20* . . ." There is no supplement to the published account of that voyage. "1819-20" should read "1821-23."

† 73° 13' N., 88° 52' W.

‡ *On Sound, and Atmospheric Vibrations* . . . (London and Cambridge, 1868, pp. 134, 135).

made in — 45° Fahr— 20° colder than it was on the 9th. In fact, the only distinctive circumstance on that date was, as remarked by Fisher, the abnormally low barometer.

THE BARISAL GUNS

The Barisal guns have no affinity to Parry's cannon—in fact, it is quite certain that they are not guns at all. The term is used to denote certain remarkable sounds resembling gun fire, but certainly not such, which occur in many parts of the world—particularly the Sundarbans, or Sunderbunds, that enormous network of swamps and morasses through which the Ganges finds its way by many channels to the sea. Barisal itself is a village in the Sunderbunds, a little westward of the principal mouth of the Ganges and about 70 miles southward of Dacca.

At Barisal, and many other places in the Ganges delta, the guns are often heard. Here is one observer's account of them:*

“I first heard the Barisal Guns in December 1871, on my way to Assam from Calcutta through the Sundarbans. The weather was calm and clear, no sign of any storms. All day the noises on board the steamer prevented other sounds from being heard; but when all was silent at night, and we were moored in one or other of the narrow channels in the neighbourhood of Barisal, Morelgunge, and upwards, far from any villages or other habitations, with miles and miles of long grass jungle on every side, the only sounds the lap of the water or the splash of earth, falling into the water along the banks, then at intervals, irregularly, would be heard the dull muffled boom as of distant cannon.

* A memorandum by Mr. G. B. Scott, quoted in *Nature*, 2. i. 1896.

"Sometimes a single report, at others two, three, or more in succession; never near, always distant, but not always equally distant. Sometimes the reports would resemble cannon from two rather widely separated opposing forces, at others from different directions but apparently always from the southward, that is seaward. We were not very far from the sea when I first heard them, and on mentioning to an old lady on board that I heard distant cannon, she first told me of the mysterious sounds known as the 'Barisal Guns.' "

Colonel H. S. Olcott* speaks of the sound heard by him, at Barisal itself, as being so sharp and loud that he thought it was the evening gun being fired at a cantonment in the village.

Mr. Scott, in the account previously quoted, also speaks of having heard similar sounds at Chilmari, on the Brahmaputra, about 300 miles inland.

"I specially remember spending a quiet Sunday, in the month of May, with a friend at Chilmari, near the river-bank. We had both remarked the reports the night before, and when near the hills previously. About 10 a.m. in the day, weather clear and calm, we were walking quietly up and down the river-bank, discussing the sounds, when we heard the booming distinctly, about as loud as heavy cannon would sound on a quiet day, about ten miles off, down the river. Shortly after we heard a heavy boom very much nearer, still south. Suddenly we heard two quick successive reports, more like horse-pistol or musket (not rifle) shots close by. I thought they sounded in the air about 150 yards due west of us over the water.

* The Theosophist. See his letter in *Nature*, 12. xii. 1895.

My friend thought they sounded north of us. We ran to the bank, and asked our boatmen, moored below, if they heard them, and if so in what direction. They pointed south!"

Similar sounds were heard on several occasions by Colonel Godwin Austen, of the Survey of India, still further inland. In the spring of 1865, while near Buxa, Bhutan, on the southern slopes of the Himalayas, "the report of a heavy gun was heard in the direction of the mountains, clear and distinct, yet a long way off, followed closely and at irregular intervals by two other discharges. . . . These reports were louder and more distinctly like artillery fire than any I afterwards heard in the hills further to the east. These last had the nature of a very, very distant boom, coming from no well-defined direction."* He also speaks of having several times heard noises, like the distant report of heavy guns, in the North Cachar Hills.

Although an Indian village is the eponym of the "Barisal Guns," India has no monopoly of them. Such noises have been heard in the British Isles—on Dartmoor, at several places in Scotland, and, quite frequently, on the shores of Lough Neagh. Here are some notes on the last—named by the Rev. W. S. Smith, of Antrim.†

"For many years after my settlement here as minister from England, I heard at intervals, when near the lake, cannon-like sounds. . . . In time I came to understand that it was not from the opposite shores, but from the lake itself that the sounds proceeded. After questioning many of the local residents, I extended my enquiries to the fishermen, but they could assign no cause. A strange thing about the matter is that

* *Nature*, 16. i. 1896.

† *Ibid.*, 2. i. 1896.

the people generally know nothing of the phenomenon, and that it is shrouded in mystery. . . . I have heard the sounds probably twenty times during the present year,* the last being on a Sunday afternoon a month since, when I heard two explosions; but with two exceptions they have all seemed to come from many miles away, from different directions at different times. They have come apparently from Toome Bay, from the middle of the lake, and from Langford Lodge Point, about nine miles distant. . . .

“I have as yet spoken to no one who observed any movement of the waters when explosions took place, nor have I spoken to any one who was close to the spot at the time, rather every one seems to have heard them only in the distance, which is strange, as fishermen are on the lake during many months in the year, at all hours of the day and night.”

Similar “guns” are often heard off the Belgian coast, where they are locally known as “mist poeffers” (*lit.* “fog-hiccups”),† while they have been reported from many parts of Australia. The earliest account of these which I can trace is that given by Sturt, when describing his great journey of 1828–9, in which he discovered the Darling and Murray Rivers. Encamped near the Darling, in February 1829, he notes in his journal:

“About 3 p.m. on the 7th, Mr. Hume and I were occupied tracing the chart upon the ground. The day had been remarkably fine, not a cloud was there in the heavens, nor a breath of air to be felt. On a sudden we heard what seemed to be the report of a gun fired

* 1895.

† See a series of articles, covering the whole range of the reported phenomena, by E. Van den Broek, of the Natural History Museum at Brussels, in *Ciel et Terre*, 1895–96.

at the distance of between five and six miles. It was not the hollow sound of an earthly explosion, or the sharp cracking noise of falling timber, but in every way resembled a discharge of a heavy piece of ordnance. On this all were agreed, but no one was certain whence the sound proceeded.

"Both Mr. Hume and myself had been too attentive to our occupation to form a satisfactory opinion; but we both thought it came from the N.W. I sent one of the men immediately up a tree, but he could observe nothing unusual. The country around him seemed to be equally flat on all sides, and to be thickly wooded: whatever occasioned the report, it made a strong impression on all of us; and to this day, the singularity of such a sound, in such a situation, is a matter of mystery to me."*

Mr. H. L. Richardson, of Hillsprings, Carnarvon, W. Australia, reported hearing three explosions high up in the air, followed by a rushing noise like escaping steam (which lasted for several seconds), on June 26, 1908.†

It is a far cry from W. Australia to the Rockies; yet here, too, we find the Barisal Guns.

On July 4, 1808, the Lewis and Clark expedition was encamped at Great Falls, Montana, about eighty miles eastward of the main range of the Rockies. The explorers record in their journal:

"Since our arrival at the Falls we have repeatedly heard a strange noise coming from the mountains in

* *Two Expeditions into the Interior of Southern Australia*, 2nd edit., 1834, vol. i. p. 98.

† *Nature*, 27. viii. 1908. See also a letter in the same paper, 4. vi. 1908, from Mr. J. Burton Cleland, describing a "dull roar, lasting several seconds," heard by him on August 9, 1907, when encamped on the Strelley River.

a direction a little to the north of west. It is heard at different periods of the day and night, sometimes when the air is perfectly still and without a cloud, and consists of one stroke only, or five or six discharges in quick succession. It is loud, and resembles precisely the sound of a six-pound piece of ordnance at the distance of three miles."

A party equipped by J. J. Astor, the American fur magnate, skirting the Black Hills of Wyoming and Dakota, noted in 1810: "In the most calm and serene weather, and at all times of the day or night, successive reports are now and then heard among these mountains, resembling the discharge of several pieces of artillery. Similar reports were heard by Messrs. Lewis and Clark in the Rocky Mountains." It may be added that in 1854 a Mr. Doty, when near the point of the Rockies from which the sounds heard by Lewis and Clark seemed to come, heard similar noises, and was certain that they emanated from the mountains. On the other hand, later visitors to the locality do not seem to have heard them.*

The Barisal Guns have also been frequently heard in Haiti,† where the sound is known as the "gouffre." They have also, though rarely, been known to occur at sea; witness the following entry in the meteorological log of the S.S. *Resolute*, Captain W. Deuchars, for July 30, 1883, 8 p.m. "Six reports like those of guns heard to the westward, supposed to be caused by electricity, as no ships are thought to be in the vicinity." The position given is $71^{\circ} 09' N.$, $12^{\circ} 28' W.$, about sixty miles westward of Jan Mayen Island.‡

* *Nature*, 26. iii. 1896.

† E.g. in the autumn and winter months of 1912, over the south-western part of the island.

‡ *Nature*, 30. i. 1896.

The guns heard at Barisal, then, appear to be only a leading case of a very widespread phenomenon.* Many explanations have been suggested—fireworks, actual gun fire, bamboos bursting in jungle-fires, thunderclaps, the collapsing of banks, globular lightning, landslips, submarine eruptions—and a good many more. So far, none of these has been accepted by those who have heard the sounds in the Sunderbunds; or for that matter, in the other localities.

Quite the most fascinating theory as to the origin of the sounds heard in the Ganges delta is that outlined by Lieutenant-Colonel W. P. Drury, of the Royal Marines, in that masterpiece of Marine fiction, *The Peradventures of Private Pagett*.† His hero, who is more of a living character than many real Marines (when these are on duty), is supposed to go adrift in the Sunderbunds in a small dinghy. He is being towed by the steam-cutter, and the painter carries away,‡ unnoticed by those ahead. He is left paddling about aimlessly like a lost dog, despairing of rescue and fortifying himself against impending death by repeating the only portion of the Prayer Book he can call to mind—which happens to be, *A Man may not marry his Grandmother*.

Round a bend in one of the innumerable channels he catches sight of a large stranded vessel. She proves to be one of H.M. ships, wrecked many years before, but still tenanted by a few greybeards commanded by a senile midshipman, the only surviving officer. All suffer from two fixed ideas—that they must never abandon the ship,

* Without extensive investigation, it is impossible to affirm that all the instances quoted are exactly on all fours, and it is not suggested that they are necessarily all due to the same cause: but they have at least a strong family resemblance—and, for all of them, no simple explanation seems adequate.

† London, 1904. See the story entitled *The Signal Guns of Gungapore*.

‡ *I.e.* the tow-rope breaks.

and that they must fire guns at intervals to scare away wild beasts. He is shown two small signalling guns, and realizes that these are the famous "Guns of Gungapore"—in other words, the Barisal guns. Parting company with these Rip van Winkles more in anger than sorrow, he makes his way, after many adventures, back to his ship—where, curiously enough, no one will believe his story.

It is a good yarn—and it is meant, I imagine, to be nothing more. Yet part of it, at any rate, might have been founded on fact. When Wallis, on his way round the world, touched at Batavia with the *Dolphin* in December 1767, he found there another King's ship, H.M.S. *Falmouth*, "lying on the mud in a rotten condition." She had been there nearly ten years. She was worn out, and so were her men. Wallis states that the ship was in so decayed a state that she could hardly be expected to survive the next monsoon—only the mud kept her from sinking at her anchors; while her ship's company consisted of no more than a few men, old and broken. There were no executive officers left, and of the remainder the gunner was dead, the boatswain had gone mad, and the carpenter was dying. The survivors entreated Wallis to discharge them from the hulk which they had tended so long, and to let them embark with him for home; offering to forfeit the ten years' pay due to them, "and go home sweepers, rather than continue the miseries of their present situation." He refused. They had Government stores on their charge, and they must await orders from England as to the disposal of these before they could quit Batavia. Poor fellows, they had never had an order of any kind from England since their arrival ten years earlier. Nothing definite is known of their fate; but Carteret, who was at Batavia in August

1768, incidentally refers to the *Falmouth* as having been condemned.

Despite the assertions of the veracious Pagett, it does not seem likely that the Barisal Guns are due to actual gunfire, or to human agency of any kind. They are probably a natural phenomenon, but whether this should be located in the air, the land, or the sea remains at present an open question. It might very suitably form the subject of an investigation to be conducted by that proposed triple combination of Forces—the Ministry of Defence.

OLD PARR, AND OTHERS

Life is very sweet, brother—who would wish to die?

SUCH was the oft-quoted question put to the despondent Borrow by Jasper Petulengro. To the speaker, of course, the answer was a foregone conclusion. He had just delivered his opinion (not, curiously enough, quoted nearly so often) about human survival:

“ . . . When a man dies, he is cast into the earth, and his wife and child sorrow over him. If he has neither wife nor child, then his father and mother, I suppose; and if he is quite alone in the world, why, then, he is cast into the earth, and there is an end of the matter.”

Assuredly Jasper Petulengro, in the prime of life, and thinking so, would not wish to die. Yet one wonders whether his wife's mother, “who died at the age of 103 and sleeps in Coggeshall churchyard,” was of the same opinion in her last years. Centenarians, so far as one can judge by what one reads about them, are chiefly occupied in boring their attendants, or reading without glasses and intelligence, and otherwise demonstrating that their faculties are no worse than they always were. If they put anything on record except a malicious codicil or so, it is usually a testimonial to the virtues of their pet nostrum, or an affidavit certifying that throughout their lives they have always shunned (or, alternatively,

eagerly consumed) tobacco and/or alcohol. Very few of them have left us any means of judging what they thought of old age, as a condition—probably they lacked the energy to do so. No doubt they speculated upon it when they were leaving their youth behind them, just as we speculate to-day; and when, if ever, we learn what they have learned, we, too, shall dismiss the knowledge as trivial and not worth recording. I have often wondered, for example, what W. K. Clifford, if he were living to-day, would have said to this passage, which he wrote in 1875, when he was a young and ailing man of thirty.

“In those cases of ripe old age not hastened by disease, where the physical structure is actually worn out, having finished its work right honestly and well; where the love of life is worn out also, and the grave appears as a bed of rest to the tired limbs, and death as a mere quiet sleep from thought; there also, in so far as we are able to realize the state of the aged and to put ourselves in his place, death seems to be normal and natural, a thing to be neither sought nor shunned. But such putting of ourselves in the place of one to whom death is no evil must in all cases be imperfect.

“I cannot, in my present life and motion, clearly conceive myself in so parlous a state that no hope of better things should make me shrink from the end of all. However vividly I recall the feelings of pain and weakness, it is the life and energy of my present self that pictures them; and this life and energy cannot help raising at the same time combative instincts of resistance to pain and weakness, whose very nature it is to demand that the sun shall not go down upon Gibeon until they have slain the Amalekites.

“Nor can I really and truly put myself in the place

of the worn-out old man whose consciousness may some day have a memory of mine. No force of imagination that I can bring to bear will avail to cast out the youth of that very imagination which endeavours to depict its latter days; no thoughts of final and supreme fatigue can help suggesting refreshment and new rising after sleep.

"If, then, we do not want to die now, nor next year, nor the year after that, nor at any time that we can clearly imagine; what is this but to say that we want to live for ever, in the only meaning of the words that we can at all realize?"*

The centenarian—the *authentic* centenarian, complete with birth certificate and other *pièces justificatives*—is quite a modern phenomenon. In former times, before the compulsory registration of births, it was often quite impossible to verify any claim of the kind. Nowadays, however, one sees accounts in the Press twice or three times a week of persons who have celebrated their 100th birthday (or who have done so and subsequently died). And there can be little doubt that most of these cases are authentic. Yet it is not so very long ago that the view was roundly asserted, and supported by a formidable body of destructive criticism, that there was no genuine case on record, supported by evidence of a kind which a Court would receive, that any known person had ever reached the age of 100 years. The leading exponent of this attitude was Sir George Cornewall Lewis (1806–63),†

* This passage is quoted from Clifford's review, "The Unseen Universe," which appeared in *The Fortnightly Review*, June 1875. To write it, he sat up all night, at a time when his health was about to break down altogether. He died of consumption at Madeira on March 3, 1879, at the age of thirty-four. He had been Professor of Applied Mathematics at University College, London, at the age of twenty-six, and F.R.S. at twenty-nine.

† See *Notes and Queries*, 1862, *passim*.

aided and abetted by W. J. Thoms and other sceptics, one of whom nearly disproved, in his own person, the correctness of his opinion.

It must be admitted that this view, although it went too far, was only a very natural reaction against the slipshod "curiosities of longevity" current at the time, and earlier. For example, "Rainy-day" Smith, in writing of various deaths which occurred in the year 1772, calmly records the following, without any details whatever.*

"(Age) 125, Rice, a cooper in Southwark; 133, Mrs. Keith, at Newnham, in Gloucestershire; 138, the widow Chum, at Ophurst, near Lichfield."

And Bailey's *Records of Longevity*† display the same uncritical spirit. In his pages the names of old folk who have reached 110 are as common as capital letters. He seems to have compiled his statistics, if one can so term them, on the theory that "if you see it in print, it is so." Hence such valuable pieces of information as the following:

"Mackarny, Susan, of Dublin, a mendicant, died 1751, aged 120.

"Moony, Catherine, near the City of Tuam, Ireland, died 1768, aged 136.

"Sharphy, William, Knockall, county of Roscommon, Ireland, died 1757, aged 138."

Nor is this all. Bailey gravely records the legendary case of one Petratsch Zartan, alleged to have been born at Kofrek, four miles from Temesvar, Hungary, in 1539; and to have died in 1724, at the patriarchal age of 185!

In dealing with such collections of rumours and hearsay, the acute and indefatigable W. J. Thoms‡ was

* *A Book for a Rainy Day* (London, 1845, p. 19).

† London, 1857.

‡ *The Longevity of Man*, London, 1879.

like a pike in a carp-pond. Penetrated with Cornewall Lewis' theory that all "centenarians" are liars, he dealt most faithfully with every case he could find, applying to each a minute and searching scrutiny which most of them were entirely unable to withstand.

But even Thoms had to admit defeat. Not only was he confronted with unexceptionable evidence as to the age of J. W. Luning (who was born on May 19, 1767, and died on June 23, 1870), but he could find no conclusive means of disproving a much more remarkable case—that of Robert Bowman, of Irthington in Cumberland, who died on June 23, 1823, and was then believed to be in his 119th year. The matter was raised in *Notes and Queries* (of which Thoms was then editor) by Canon C. G. V. Harcourt in 1870:* and inquiries specially set on foot by Thoms with the object of exposing a mare's-nest resulted only in showing that, while the evidence of Bowman's great age was not perfectly conclusive, the presumption was in favour of it, and that it could only be set aside if one were to hold, *a priori*, that such a span of life was absolutely impossible.† Such, of course, was Thoms' conviction; and he ". . . said it very loud and clear," as follows:

"(some may) believe it. I do not: and in the absence of direct and more satisfactory evidence to the contrary shall continue to assert that ROBERT BOWMAN WAS NOT 118."‡

* *Loc. cit.*, 4th Ser., vi. 91.

† As an example of Thoms's rigorous but rather irritating methods, it may be noted that he accepted Luning's marriage certificate (dated August 4, 1796) as referring to Luning (still living, 1869) but not as referring to the child born in May 1767 (which was also Luning). What finally removed his doubts was the fact that Luning had insured his life with the Equitable Co. when he was thirty-six, and had maintained the policy ever since. As Thoms admits ". . . No man ever makes himself older than he is when effecting an insurance, and few live seventy-seven years after it."

‡ *Loc. cit.*, p. 207.

Needless to say, Thoms would not hear a word in favour of what are generally regarded as the three leading cases of enormously-prolonged human life—those of Old Parr (152?), Henry Jenkins (169 or 160?), and the Countess of Desmond (140?). His criticism, granting his peculiar views, was perfectly fair, and he certainly demonstrated the now-accepted fact that there is no really valid evidence which can be adduced in support of any of them; but for the first and third there is still enough to interest any but the most hardened sceptic—and it must be remembered that the lack of conclusive evidence (often occasioned by adopting an impossibly high standard of such evidence) is not by any means the same thing as conclusive disproof. On the other hand, there is hardly any subject upon which it is easier to create a personal “legend.” Let it once be believed that some person has neared or reached the century, and the neighbours, friends, and relations, shining with reflected glory, will make it an article of faith, and justify it by all manner of strange arguments, after the fashion of Smith the weaver.*

I propose to give a short outline of the known facts about Old Parr, the Countess of Desmond, and C. J. Drakenberg (1626–1772). I omit Henry Jenkins, because I cannot think that he stands on the same footing as the other three. There is, literally, not a particle of evidence for his age except his own unsupported and contradictory

* CADE. . . . The elder of them, being put to nurse,
Was by a beggar-woman stol'n away;
And, ignorant of his birth and parentage,
Became a bricklayer when he came to age:
His son I am; deny it if you can.

DICK. Nay, 'tis too true; therefore he shall be king.

SMITH. Sir, he made a chimney in my father's house, and the bricks are
alive at this day to testify it; therefore, deny it not.

King Henry VI, Part II, Act IV, Sc. 2.

assertions—made when he was an old man, past work and depending for a living on the charity of the curious. He had an active interest in maintaining the “Jenkins legend,” and it is significant that Charles Anthony, vicar of Catterick, who buried Jenkins on December 9, 1670, and who is described as “a strict exact man, and evidently a very careful parish priest,” merely entered him in the register (of Bolton-on-Swale) as “a very aged and poore man,” and refrained from making any mention of his age. This can scarcely have been an oversight, since Jenkins had given evidence in Anthony’s favour in a tithe case at Catterick some years earlier, and had then described himself as being 157.

OLD PARR

In dealing with “Old Parr” it is best to begin the wrong way round. There is no doubt at all as to when he died, but the date of his birth is uncertain.

Thomas Parr died in London on November 14, 1635. Until a couple of months before, he had spent his whole life at Alberbury, near Shrewsbury; being, in effect, a standing proof that Professor A. E. Housman’s phrase about “. . . the lads who will never grow old” is of limited application, even in Shropshire.

In the spring of 1635 Thomas Howard, Earl of Arundel, then on a visit to his estates near Shrewsbury, happened to hear of Parr as having reached the age of 152. He visited him (Parr was then blind and, not unnaturally, rather feeble), and “the report of this aged man was certified”—by what particulars does not appear. Howard determined to bring him up to London, for the delight of the Court; and did so, in a litter, by easy stages.*

* Winnington - Wem - Shifnal - Wolverhampton - Birmingham - Coventry - Daventry - Stony Stratford - Redbourn - London. The *cortège* averaged about

He was presented at Court, where tradition relates that Charles I put to him the rather embarrassing question, "You have lived longer than other men—what have you done more than other men?" and received the blunt reply, "I did penance (for incontinence) when I was an hundred years old."* For some weeks "the old, old, very old Man," as he came to be called, was exhibited at the Queen's Head in the Strand; where, although in no need, he continued to occupy himself with "twisting of small lines, and Cords . . . and this hee doth with an apprehension that it getteth him money."† But the change of air, diet, and mode of living, together with the constant disturbance and excitement occasioned by curious sightseers, were too much for the old man, and he withered in London like an uprooted tree.

William Harvey, famous as the discoverer of the circulation of the blood, made a complete examination of Parr's body on November 16, 1635, in the presence of several others of the King's physicians. He drew up a very interesting report, which is printed in his *Works*. Apparently he could find no unequivocal indications of great age; in fact, from the wording of his account the body might have been that of a man of between sixty and seventy. There is nothing of general interest in the medical details. Harvey concludes thus:

eighteen miles a day. Why they went round by Wem to reach Shifnal (where Howard had a seat) is obscure; they could have saved nearly ten miles by going through Shrewsbury. At Coventry Parr was mobbed by the local rubber-necks, which greatly distressed him.

* How this would have delighted Charles II—who, however, was then only five years old.

† Quoted from a broadsheet headed "THE WONDER OF THIS AGE," issued two days before Parr's death, and containing a well-engraved portrait (see Plate III), a little biographical information, and a good deal of clap-trap. On a copy in the British Museum the engraver has added, in a vacant space at the foot of the portrait, "He dyed November the 15th (sic) And is now buried in Westminster 1635."



*The Olde, Old, very Olde Man or Thomas Parr, the
Sonne of Iohn Parr of Winnington in the Parish of Alberbury:
In the County of Shropshire who was Borne in 1483 in
The Raigne of King Edward the 4th and is now living in
The Strand, being aged 152 yeares and odd Monethes 1635
He dyed November the 15th And is now buryed in Westminster: 1635*

PLATE III

THOMAS PARR

NOTE.—This is reproduced from a copper-plate engraving published, as a broadside, two days before Parr's death. It will be noticed that the engraver has added the date of this subsequently

"All the internal parts, in a word, appeared so healthy, that had nothing happened to interfere with the old man's habits of life, he might perhaps have escaped paying the debt due to nature for some little time longer.

"The cause of death seemed fairly referable to a sudden change in the non-naturals, the chief mischief being connected with the change of air, which through the whole course of life had been inhaled of perfect purity . . . but in this great advantage . . . this city is especially destitute: . . . a city whose grand characteristic is an immense concourse of men and animals, and where ditches abound, and filth and offal lie scattered about, to say nothing of the smoke engendered by the general use of sulphureous coal as fuel, whereby the air is at all times rendered heavy, but much more so in the autumn than at any other season. Such an atmosphere could not have been found otherwise than insalubrious to one coming from the open, sunny and healthy region of Salop; it must have been especially so to one already aged and infirm.

"And then for one hitherto used to live on food unvaried in kind, and very simple in its nature, to be set at a table loaded with variety of viands, and tempted not only to eat more than wont, but to partake of strong drink, it must needs fall out that the functions of all the natural organs would become deranged. . . .

(and so on, in a manner very commendable, and deserving the attention of the New Health Society).

". . . The brain was healthy, very firm and hard to the touch; hence, shortly before his death, although he had been blind for twenty years, he heard extremely well, understood all that was said to him, answered

immediately to questions, and had perfect apprehension of any matter in hand; he was also accustomed to walk about, slightly supported between two persons.

“His memory, however, was greatly impaired, so that he scarcely recollected anything of what had happened to him when he was a young man, nothing of public incidents, or of the kings or nobles who had made a figure, or of the wars or troubles of his early life, or of the manners of society, or of the prices of things—in a word, of any of the ordinary incidents which men are wont to retain in their memories. He only recollected the events of the last few years. Nevertheless, he was accustomed, even in his hundred and thirtieth year, to engage lustily in every kind of agricultural labour, whereby he earned his bread, and he had even then the strength required to thrash the corn.”

Parr was buried in Westminster Abbey, under a stone recording that he had lived in the reigns of ten monarchs—Edward IV to Charles I, inclusive.*

And now, when was he born?

The only contemporary authority for his life is a pamphlet† by John Taylor, the “water-poet,” which must have been “yarked up” in great haste, for it was on sale in London on November 12, 1635, two days before Parr’s death. It is dedicated to Charles I, and is partly in prose and partly in somewhat pedestrian verse—rhymed couplets, such as the following:

* The inscription was recut in 1870. Parr is also commemorated by a brass in his parish church of Alberbury.

There is no record of his burial in the Westminster Abbey registers. Before 1660 these are very imperfect—there are only two interments recorded as having taken place in 1635.

† *The Old, Old, Very Old Man: or the Age and Long Life of Thomas Parr . . .*, by John Taylor. London: printed for Henry Gosson, at his Shop on London Bridge, neere to the Gate. 1635.

“. . . He was of old *Pithagoras* opinion
That greene cheese was most wholesome (with an
onion) . . .”

In view of what Harvey says about Parr's impaired memory, one can only imagine that Taylor got his facts—if they are facts—from some of the old man's attendants. They have never been verified, and no new evidence has since come to light.

According to Taylor, Thomas Parr was the son of John Parr, of Winnington,* and was born in 1483. The method used by Taylor in computing his dates was based on particulars of the various leases granted to Parr in respect of a small holding which had passed to him from his father, and which he continued to cultivate throughout his long life. Reduced to prose and tabular form, they are as follows.

<i>Age of Parr.</i>	<i>Date.</i>	
17	1501	John Parr obtained a twenty-one years' lease of his holding from Lewis Porter.†
35	1518	John Parr died. Thomas Parr, then in service, returned to Winnington to enjoy the remaining four years of the above lease.
39	1522	Parr obtained a second lease of twenty-one years from Lewis Porter.
60	1543	Parr obtained a third lease of twenty-one years from John, son of Lewis Porter.
81	1564	Parr obtained a fourth lease of twenty-one years from Hugh, son of John Porter.
102	1585	Parr obtained a lease for the term of his life from John Porter, son of Hugh Porter above. This he enjoyed for fifty years.
152	1635	Death of Thomas Parr.

* A hamlet in the parish of Alberbury, nine miles 280° from Shrewsbury.

† This is inferred.

In connection with the life-lease, an anecdote of Taylor's will bear retelling:*

"One remarkable passage of the old man's policy must not be omitted or forgotten, which is thus; his three leases of sixty-three years being expired, he took his last lease of his landlord, one Master John Porter, for his life; with which lease, he hath lived more than fifty years; . . . but this old man would, for his wife's sake, renew his lease for years,† which his landlord would not consent unto; wherefore old Parr, having been long blind, sitting in his chair by the fire, his wife looked out of the window, and perceived Master Edward Porter, the son of his landlord, to come towards their house, which she told her husband, saying, 'Husband, our young landlord is coming hither.'

"'Is he so?' said old Parr; 'I prithee, wife, lay a pin on the ground near my foot, or at my right toe,' which she did; and when young Master Porter (yet forty years old) was come into the house, after salutations between them, the old man said, 'Wife, is not that a pin which lies at my foot?' 'Truly, Husband,' quoth she, 'it is a pin indeed'; so she took up the pin, and Master Porter was half in a maze that the old man had recovered his sight again; but it was quickly found to be a witty conceit, thereby to have them to suppose him to be more lively than he was; because he hoped to have his lease renewed for his wife's sake, as aforesaid."

* I have modernized the spelling.

† Under a lease for his life, and not for a specific term, his wife, at his death, must have relinquished the holding. He had no legal offspring, but if he were a "tenant for years" his wife could have inherited the unexpired portion of the lease, as Parr did from his father.

The date of this interview is not stated by Taylor, but it was probably towards the end of Parr's life-lease, for he did not go blind until 1616.* We can scarcely blame John Porter the younger for refusing a further twenty-one years' lease to a tenant of over 130; although as he had already run through three and a bit (as well as thirty years of a life-lease), and had held two of them from Porter's great-grandfather, it would not have been surprising if Edward Porter had considered that Parr was not unlikely to see his father and himself under the ground also.

The wife referred to above was Parr's second venture. Apparently he remained a bachelor until he was eighty; after which, grown reckless, he married Jane Taylor, by whom he had a boy and a girl (both of whom died in infancy). His married life was not entirely uneventful, for in 1588 he is supposed (as he boasted to Charles I) to have done penance, in a white sheet, in Alberbury Church for having begotten a bastard child by one Catherine Milton. In 1595 he buried his first wife, but with unshaken nerve he took a second, Jane Lloyd (or Flood) ten years later—by whom, not unnaturally, he had no offspring:† a fact which, as he had no surviving children by his first wife, somewhat embarrasses the

* Peck, *Collection of Curious Historical Pieces* (London, 1740, p. 51).

† In fairness to Parr, however, Harvey's testimony on this point should be quoted: "The organs of generation were healthy . . . (*desunt nonnulla*); so that it seemed not improbable that the common report was true, viz. that he did public penance under a conviction for incontinence, after he had passed his hundredth year; and his wife, whom he had married as a widow in his hundred-and-twentieth year, did not deny that he had intercourse with her after the manner of other husbands with their wives, nor until about twelve years ago had he ceased to embrace her frequently." In view of the foregoing, and of his exploit in the Armada year, it would be rash to assert that he left no surviving descendants. Not improbably those who have claimed to be such took a pride in their left-handed pedigree: just as Davenant eagerly passed for one of Shakespeare's love-children, and the Maltese boatman tells you with pride that he is descended from the Knights of Malta—a celibate order.

historian who is compelled to record that an alleged great-grandson, Robert Parr, died at Mitchelstown in July 1761 (aged 127?), and an equally alleged great-granddaughter, Catherine Parr, at Skiddy's Almshouses, Cork, in October 1792 (aged 103).^{*} It may be noted that Ireland shares with Russia the distinction of having provided the largest number of utterly baseless reports of extremely long-lived persons.

As already stated, Taylor is practically the sole authority for the dates of Parr's history, which entirely lacks any definite confirmation. But there are two points to be noted. Unlike Jenkins, Parr (if we accept Harvey's account) did not profess to be an authority as to his own age. Owing to his impaired memory, Taylor, Howard and the rest must have obtained their information about him from other sources. We have no right to assume that they made no critical investigation whatever at the time—and they were certainly (if, perhaps, easily) satisfied. If Parr had been simply a man of seventy or so, as he appeared to be by Harvey's examination, it is scarcely credible that his neighbours could have endured to see him so civilly treated by the quality, provided with a litter and attendants, and carried off to distant London with every circumstance of respect, and in a state far above that to which his humble circumstances entitled him—it is scarcely credible, I say, that they could have seen all this and not have yielded to the temptation to put a spoke in his wheel. And if, on the other hand, he were at least very much older than his neighbours—say, upwards of a century old—then the physical condition of his body was certainly abnormal, and his real age may have been equally so. "Hard cases make bad law."

Admittedly, conclusive evidence as to Parr's century

^{*} See the *Annual Register* for 1792.

and a half of life is lacking; on the other hand, that he was generally regarded, in the last years of his life, as phenomenally old is a fact which admits of no dispute. We may, if we please, hold that this was occasioned simply by the strong love of the marvellous which is innate in most of us; or we may merely consider that the story, intrinsically improbable, is now incapable of conclusive proof or disproof, and should be regarded as "not proven." But, in my submission, we cannot call it incredible.

KATHERINE, COUNTESS OF DESMOND

As in the case of Old Parr, it is best to begin the story of "the old Countess of Desmond" at the date of her death. Since she was of Irish lineage, the procedure is peculiarly appropriate.

Katherine Fitzgerald, wife of Thomas, twelfth Earl of Desmond, is believed to have died in 1604 at Inchiquin Castle, near Youghal, Co. Cork.* The cause of her death is uncertain†—in all likelihood it was due to old age, for she was supposed to be about 140.

Although stories as to her very great age had been current for many years before her death, the first more or less definite statement on the subject is that published by Fynes Morison in 1617‡—thirteen years after it. He records that "in our time" she had lived to the age of "about" 140, although she was able, even in her last years, to go on foot to the market-town three or four miles distant once a week. There is little doubt that when Morison visited Youghal in 1613 he must have

* The authority for this is a MS. (No. 626) in the Lambeth Library.

† Various apocryphal stories have been told of her dying as the result of a fall from an apple, walnut, or cherry tree.

‡ In his *Itinerary*.

met with many who had personal knowledge of her. Contemporary writers, such as Bacon, Raleigh, and Archbishop Ussher,* accepted and repeated his statement. Raleigh, in his *History of the World* (published 1614), refers to her as having been living in 1589 "and many years since": a fact to which he could ruefully testify, for in 1588 and 1589 he had to grant certain leases at a reduced rent pending the life of "the ladie Cattelyn, old countess dowager of Desmond," who had a life-interest in the lands. And in Sir John Harrington's *Short View of the State of Ireland*, written in 1605 but not published until 1879, occurs the following remark, apropos of the Irish climate: ". . . where a man hath lived above 140 year, a woman, and she a countess, above 120, the country is like to be helthy." The man has not been identified; nor can I say with certainty why Harrington considered that countesses, as a class, were generally short-lived.

Unfortunately, the date of the Countess' birth is unknown; and that of her marriage has not yet been definitely established. The facts are these. Her husband, the twelfth Earl of Desmond, had previously been married to Sheela, daughter of Cormac McCarthy. This lady is known to have been living in 1505, for documentary evidence exists that on June 9th in that year Gerald, eighth Earl of Kildare, granted her† a lease of certain lands for five years. On the other hand the Earl, who died in 1534 at the age of eighty, had a daughter by his second wife, the "old countess," and there is therefore a

* Author, *inter alia*, of the long-accepted chronology of Scripture found in most Bibles. He determined the date of the Creation as Sunday, October 23, 4004 B.C. He refers to the "Old Countess" as having been in his time "both living and lively."

† Under her Irish style of Gilis ny Cormik. At that period the Irish language, not being compulsory, was in common use in Ireland.

presumption that his remarriage took place some considerable period before his death.

On the known facts, the age of "the old countess" can never be arrived at with certainty. Any result which one reaches depends entirely upon the view taken as to relative probability of two events, both *prima facie* improbable. "The case stands thus." To the seventy years (1534-1604) which the countess survived her husband, we must add (*a*) her probable age at the time of her marriage, and (*b*) her daughter's age at the time of her husband's death. An advocate, such as Thoms, actively interested in reducing her age to its lowest possible amount, might assume for (*a*) and (*b*) the values of 16 years and 0 months respectively. In this way he would make the countess 86 at the time of her death. On the other hand a credulous author of the Bailey type might take (*a*) as 45, or even 50, and (*b*) as 28, which would bring her age out at 148.

The truth lies somewhere between these two extremes; where, is a matter of pure assumption. A vigorous husband of eighty is almost as unusual a phenomenon as a Transit of Venus; still, women of 140, or even 120, are not as plentiful as blackberries.

As a matter of fact, Thoms concedes that the Countess "was probably about a hundred . . . at the time of her death." Adopting this opinion of the *advocatus diaboli*, stern fact compels us to add another twenty-three years or so to it—for this reason. He took the date of the grant to Desmond's first wife as being "20 Henry VIII"; i.e. 1528. The correct date, however, is "20 Henry VII," or 1505. Since his time the document, on which the date is plainly legible, has been published in facsimile*—and it has also been pointed out that the

* *Journal of the Kilkenny Archaeological Society*, new ser., iv. III. 1864.

Earl of Kildare who made the grant in question died in 1513.*

It is safe to conclude that the Countess of Desmond had long exceeded the century when she died, and that an arguable case could be made out for her having attained the age of 120. There is no conclusive proof that she did not reach the span claimed for her—140—but, equally, there is nothing but hearsay to be cited in support of it. It has, it is true, been stated that “documentary evidence of the Countess of Desmond’s age is said to exist”—but I fancy that this statement is based merely on a letter, signed “A Resident in the County of Waterford,” which appeared in *The Times* of May 24, 1872. The writer declared that it was in his power to confirm the statement (as to the Countess being 140), for “a landlord in the county of Waterford has in his possession a legal document of the time of James I, wherein it is set forth that certain lands would fall in on the death of the Countess of Desmond, now aged seven score years.” The following day, a letter from Thoms appeared, inviting him, in the interests of historical truth, to furnish particulars of this extremely curious document—but none such were forthcoming. In any case, the instrument in question would only provide additional, and superfluous, proof that the Countess was reputed to be “seven score years” old at or about the time of her death.

CHRISTIAN JACOBSEN DRAKENBERG

In the case of C. J. Drakenberg, it is possible to give a straightforward account of his long life, since the dates of his birth and death, and of the principal events of his

* As Swift has put it:

“Who killed Kildare? Who dared Kildare to kill?
Death killed Kildare—who dare kill whom he will.”

history, are fairly well known.* But, as will be seen, there is a "veiled period" in his life, and its later years are not free from a suspicion of personation.

Drakenberg was born at Blomsholm, Norway, on November 18, 1626. As a Norseman should, he went to sea; we find him, at the age of eleven, sailing in his Dutch uncle's† ship. He continued to follow the sea uninterruptedly until about 1694, making voyages to Greenland, North and South America, and the Mediterranean, as well as serving two terms, one of three and the other of six years, in the Danish Navy during the chronic wars between that country and Sweden.

About 1694, while on a voyage to Cadiz for a cargo of wine and salt, his ship was captured by Algerine pirates, then and for more than a century afterwards the scourge of the Mediterranean. As was the custom, the captured crew were sold into slavery, and Drakenberg remained in this condition, serving various masters, for some sixteen years. In 1710 he was at Aleppo, in the household of a rich Jew, and it was then that, having made a friend at Court, he scented an opportunity to escape. The friend in question was a Swede named Stephen Johansen Ert, apparently a renegade ex-slave, who had obtained a measure of liberty (and freedom to trade) as the price of his "conversion," and who had turned his opportunities to such useful account that he had been able to remit considerable sums to Sweden. He had recently obtained leave to go there (pledging his honour, for what it may

* Drakenberg seems to have escaped the notice of most writers on the subject of longevity. I have found short notices of him here and there, in various books; but the best which I have come across is an article by Miss C. Fox-Smith, which appeared in *The Blue Peter* for March 1927, and which I have used as the basis of my account of him. I am bound to say, however, that it contains some internal contradictions in matters of chronology.

† The uncle seems to have lived up to the disciplinary traditions of his species, for after three years Drakenberg transferred to another Dutch skipper's vessel.

have been worth, that he would return) and had booked a passage on board an English ship lying at Iskanderun.

Drakenberg, and five of his fellow-captives, won Ert over, and with him the English skipper. The details were soon arranged. Drakenberg's master, who was on friendly terms with Ert, came on board to see him off, accompanied by Drakenberg and two other slaves. The captain and Ert plied the unfortunate Jew so hard with farewell toasts that he was soon under the table. A boat, armed and provisioned, was standing by; a signal was made to the other three prisoners; and in a short time the refugees were heading seaward. Coasting the shore by night, and sheltering from pursuit as best they could in the daytime, they made their way to Malta, losing one of their number en route through privation and exposure. From Malta, Drakenberg made his way to Bordeaux, where he joined a ship bound to Arendal, in Norway.

Although now eighty-five, Drakenberg continued to follow the sea. He made a voyage to Spain, and then enlisted in the Norwegian Navy. The story is told, that while so engaged he had a narrow escape of being hanged. He met Norway's greatest naval hero—the almost legendary Tordenskjold, then a lieutenant—in the streets of Oslo, and neglected to salute him. Tordenskjold, in true Zabern style, struck him with the flat of his sword, whereupon the fiery old sailor, who was always notorious both for his strength and his fits of berserk rage, whipped it out of his hand and threw it over the roof of a house. He was promptly laid in irons, but Tordenskjold let him off with a caution.

After some three years with the fleet, he went back to the merchant service, and was wrecked off Husum in the great gale of December 1717. From 1718 to 1720 he

was in Holland. Then he visited Sweden, and afterwards made a long stay, as an honoured guest, with one Lieutenant-Colonel Berregaard at the latter's estate of Orsevkloster in Denmark. By this time his age—he was 100 in 1726—was beginning to bring him fame, if not fortune. It won him, for example, an interview with King Christian VI, who is stated to have given him the rank and pay of a naval boatswain and the price of a new suit of clothes.

After reaching his hundredth year, he “swallowed the anchor,” and the sea knew him no more. He found a patron, one Count Danneskiold Samsøe, who provided him with a house at Aarhus, and benefited him in many ways. As some slight return, Drakenberg made a journey to his birthplace, Blomsholm—no light undertaking, even for a much younger man—in 1732-3, and brought back with him, for the satisfaction of one of Samsøe's guests, documentary evidence of his remarkable age; to wit, a baptismal certificate signed by the parish priest.

In 1736, being then in his 111th year, he married a mere child of sixty, whom he survived many years. With all a sailor's recklessness, he did his best to find a second consort—but his luck held to the end.

He died at Aarhus in October 1772, aged 146 years and eleven months, and was buried in Aarhus Cathedral. As Lenin does now, he lay in state, in an open coffin, for many years—until 1840.

Such is the story of Christian Jacobsen Drakenberg, that ancient mariner. At first sight, it seems a first-class “news-feature.” The tale is supported by quite a lot of evidence, and the dates of birth and death will stand detailed scrutiny. But the natural question arises—were there two, or even more, Drakenbergs? Was the

Drakenberg who died in 1772 the same man who saw the light in 1626?

There may have been two of him. Clearly, the weakest link in the chain is the period of Drakenberg's sixteen years' captivity in Algiers. He was already sixty-eight or so when captured, and would have been eighty-four when he escaped. Did he die in the bagnio, and did someone else assume his identity?

Miss C. Fox-Smith has suggested that such may have been the case. She has even identified the supplanter as the merchant S. J. Ert (or, should we, in view of his somewhat mysterious character, read this as "Ert, S. J."?). Here is her presentation of the case:

" . . . He (Ert) fills the bill in more ways than one. He was something of a linguist, and we are told that Drakenberg in his old age spoke several languages well. He desired to revisit Sweden, which country Drakenberg actually did visit very soon after his escape. The fact of his having been amassing money in Sweden explains his anxiety to regain his liberty, while his promise to the Sultan to return provides a sufficient reason for getting rid of his own identity. He must, moreover, have had ample opportunity to familiarize himself with the main points of Drakenberg's early career. Men thrown together in captivity soon become intimate; and we are also told that Ert had been accustomed to act as letter-writer in ordinary to his fellow-prisoners, which would have afforded him ample opportunity to find out personal details about them.

"But—it may be objected—how would it be possible for a man of forty or fifty to step into the shoes of one of ninety? Easily enough, says our sceptic: men age very quickly, as everyone knows, in slavery, and it is,

further, specifically stated by those who knew Drakenberg after his return from Asia that he 'might easily have been taken for a man of sixty.' "

And she goes on to suggest that Ert or another, finding that Drakenberg's fame as a later Old Parr was a useful asset, kept up the deception (originally meant to be soon discarded) until his death; just as the Tichborne claimant found that every step in his career of imposture led him on to another.

I confess that I cannot follow this argument. I pass over the perfectly delightful specimen of a fallacy to be found in the remark about Drakenberg's linguistic powers; and I need not stress the point that Drakenberg did not, apparently, visit Sweden "very soon after his escape," but some ten years later. The main objections seem to me to be these.

What had Ert to gain by saying he was Drakenberg? So far as I can see, nothing whatever. Drakenberg would have had much more reason for saying that he was Ert. Ert was a man of substance, going home to enjoy large sums which he had remitted to Sweden. What on earth could induce him to do his utmost to forfeit his rights in them by announcing on his arrival that he was not Ert, but a poor seaman named Drakenberg? After his long absence, the slightest suspicion that he was not S. J. Ert, of Aleppo, would have made it very difficult for him to recover his money—and what would be more likely to breed such suspicion than any attempt to personate a man many years older, and of different speech, education, and profession?

Even supposing (which I cannot) that Ert could carry off his impersonation successfully, the question of motive remains unanswered. What had he, in Sweden, to fear

from the Turks? And, if the Sultan's arm could have reached him there, how would he better his lot by ceasing to be Ert—a merchant overstaying his leave—and becoming Drakenberg, a runaway slave who had escaped from the Sultan's dominions, probably not without violence?

Lastly, even if we suppose that Ert, for no assignable motive, decided that he would ply the trade of modern Methuselah at the sign of the Drakenberg's Head, the fact remains that there was no money in that trade for a long time after he escaped from Aleppo. Drakenberg—or Ert—was at sea for several years more, and faring by no means sumptuously. If a clever man like Ert, in the prime of life, was content to serve, as Jacob did, seven years on the off-chance that he might later on be able (if he lived) to beg a pittance as a freak—so be it: one cannot argue about the motives of a man who acts in such a way, for he is evidently insane.

It will be gathered that I do not believe that the Drakenberg of 1710-72 was Ert. There is no evidence that he was any other pretender. On the facts, I consider that the balance of evidence is in favour of the view that the Drakenberg of 1626 was the Drakenberg of 1772—the hiatus in his history notwithstanding.

Conclusion.

Many more cases of abnormal longevity could be cited, but space forbids. I have tried to give an outline of the evidence (such as it is), which goes to show that in one or two very exceptional cases human life may extend longer than is generally thought possible. Lovers of the marvellous, if they are uncritical, can easily find stories much more wonderful. In addition to Petratsch Zartan (185?), already referred to, here is a list, in Bailey's

manner, of cases which are periodically given an airing in print by the snippet-journalist. They are all entirely unsubstantiated.

			<i>Obit.</i>	<i>Ætat.</i>
William Edwards, Cardiff	1668	168
Jonas Warren, Baldoyle	1787	167
Louisa Truxo, Brazil	1780	175
Jean Korin, Hungary	—	172
Thomas Caron, Shoreditch	1588	207

In the last case, that of Caron, it is quite certain that the age originally inscribed on his tombstone was 107, and that this was altered at a later date.

According to the Press,* we can still find people living who are 150 and upwards—generally, and not unnaturally, in regions where life-assurance agents find their own lives rather insecure, and the recording of births and deaths is performed, mentally, by the oldest inhabitant. Thus in 1926 we were informed that one Zaro Agha, of Constantinople, aged 150, was undoubtedly the “world’s oldest.” Not long afterwards, it was reported that the results of a census taken in Angora had revealed the existence of a woman, named Fatma Handum, who was 160.† Nor was the unfortunate Zaro Agha long permitted to enjoy even the lesser distinction of being the oldest man in the world, for a report soon afterwards arrived from Sarajevo—much less unhealthy, apparently, for peasants than for Archdukes—that Tadija Moustafitch, a native of Herzegovina, claimed to be 156.‡

* The following misprint, in *The Times* of November 13, 1872, suggests, at first sight, a rather high “expectation of life”:

A RESPECTABLE YOUNG PERSON WANTED,
Age about 81 or 19, as HOUSEMAID, and to wait
at table. Apply between 10 and 12 at 40, Queen’s Road,
Bayswater.

† *Daily Express*, March 23, 1927.

‡ *Evening Standard*, July 18, 1928.

Such reports, from their *ex-parte* character and lack of detail, do not inspire much confidence—nor, indeed, is it to be expected that they should. Yet it is scarcely fair to regard them as intrinsically impossible. It is perfectly true that most recorded instances of abnormal longevity are based upon the flimsiest evidence, or none; and that even the few to which this objection does not apply are based upon a mixture of evidence and assumption; but it is one thing to regard them, if we wish, as “not proven”—it is quite another to draw an arbitrary line (whether it be at 100, or 110, or even 120 years) and dogmatically assert that it is, in the nature of things, utterly impossible that human life should ever extend beyond this limit.

As an analogy, consider the case of abnormal height. There is a well-defined average height for most races, just as there is an average duration of life; and, if we look only at averages, there is a well-marked upper limit to both. A man 6 feet 3 inches or so is as much above the average as a man of seventy-five, and one of, say, 6 feet 10 or 11 about as uncommon as a centenarian. Above 7 feet we get into the region of the definitely abnormal, and the percentage of such cases in relation to those of normal height is so small as practically to defy analysis. Nor do such cases appear to follow any very clear law—for example, there does not seem to be any definite relation between the numbers of persons who reach 7 feet and those who reach 8 feet. And if such a law could be found, it is fairly certain that predictions based upon it would not be of much value. For example, I am confident that analysis of the authentic cases of gigantism recorded between 1800 and 1900 would have led to a reasonable conclusion that no human being could ever reach the height of 9 feet—or, at least, that the chances against this ran into such astro-

nomical figures that it could be dismissed as a total impossibility. And yet, soon after 1900, two separate instances of this height being exceeded came to light. Machnov, the Russian giant, who was over here in 1905, stood 9 feet 3 inches, and Jan van Albert, who visited London in 1924, was 9 feet $3\frac{1}{2}$ inches.

Now, even if we discard the traditional years of the Psalmist, and say, with the son of Sirach, "The number of a man's days at the most are an hundred years,"* a man of 152 is not proportionately so much beyond the ordinary limit as one of 9 feet 3 inches.† And similarly no amount of statistical evidence could, I suggest, prove definitely that such a span of life was a physical impossibility.

Why one or two individuals should survive—if they do survive—so much longer than their fellows is a question to which there is no clear answer. It may be due to some small organic abnormality—just as gigantism is due to an abnormal condition of the pituitary gland. It does not seem to depend much upon diet, or height, or build, or sex—or even upon locality. It certainly is not due to any nostrum such as the notorious "Old Parr's Life Pills,"‡ which Bon Gaultier celebrated in verse,§ and which have still, I believe, a certain sale among the poorer classes. Those better able to indulge a yearning for longer life and renewed vitality no doubt prefer the "monkey-gland" treatment of Dr. Voronoff—a process whose somewhat repulsive details are, perhaps, best not further discussed in print here.

* *Ecclesiasticus* xviii. 9.

† 6 feet: 9 feet 3 inches as 100 years: 154 years approximately.

‡ These are supposed to be compounded upon a secret recipe obtained from one of Parr's descendants (but actually evolved by one T. Roberts, a Manchester druggist). See the *Medical Circular*, 23. ii. 1853 and 2. iii. 1853.

§ In his *Ballad of Parr's Life Pills*—one of his seven examples of the "puff poetical."

There is, however, more to be said for the views enunciated by Mechnikov.* It is undoubtedly true that, broadly speaking, normal longevity diminishes as we go up the scale of life. Organisms such as the amœbæ, which multiply by fission, are (in a sense) immortal; reptiles, on the whole, live longer than birds, and birds longer than mammals.† Mechnikov connected this, somewhat curiously, with a fact of his own observation—that Russian peasants, who are accustomed to drink large quantities of sour milk, are notably long-lived. He pointed out that the part of the digestive apparatus which temporarily retains waste matter is relatively much larger in reptiles than in fishes, and in birds than in reptiles; and that it reaches its greatest proportionate development in mammals. He assumed that this part, which is of little importance in digestion, is the seat of extensive putrefactive changes whose products, reabsorbed by the blood, cause auto-intoxication, and in time favour the advance of senile decay. On this ground he considered that we should, if we wish to live long, prevent intestinal putrefaction—which can be effected by employing the bacilli which occur in sour milk. Accordingly, he recommended a diet which should include preparations of milk soured by cultures from selected lactic acid—a diet which, I believe, is still favoured by a great many people, although Ilya Mechnikov himself died in 1916 at the comparatively early age of 71.

It may be so. In this or some similar way we may succeed in prolonging our lives, or at least in thinking that we have done so. But, unless we can at the same time ward off the physical changes accompanying old

* See, in particular, his *Nature of Man*, 1903.

† There is, I believe, a tradition to the effect that some mediæval forerunner of Darwin was burned for maintaining that an Archbishop is a mammal.

age, what use would it be? Turn to those few immortal pages in which Swift tells of the ghastly 'life-in-death' of the Struldbruggs, those wretched creatures who could not die. It is easy to say, as Browning did:

Grow old along with me,
The best is yet to be,
The last of life, for which the first was made . . .

but is it really true? We have all met wonderful old men and women—the particular example in my mind is Admiral of the Fleet Sir Provo Wallis,* the only admiral to celebrate his hundredth birthday while still on the Active List—in whom old age really is the crown of life; but they are a small minority. Too often the closing years are spent in increasing infirmity and pain, for which there is no remedy but death. Often, too, the real personality dies years before the animal residue. Who can think, unmoved, of such a fate as that of Swift himself, or of Nietzsche, or of a very great lady who died not long ago? Man's body may, perhaps, survive to a century and a half; but in doing so the man himself becomes slowly but surely an object first of curiosity and then of pity; so that he comes to answer Petulengro's question as Borrow himself did—*I would wish to die*.

* He died in 1892, aged 100 years 10 months. By an Order in Council (22.2.1870), he was retained on the Active List, in consideration of his past services, for life. He had been first-lieutenant of the *Shannon* when she took the *Chesapeake* in 1813. Incidentally, James Coull, who lost an arm at the *Shannon's* wheel during the engagement, lived to be 94.

THE LANDFALL OF COLUMBUS

[“Landfall” * is a “term of endearment used by seamen” to denote the first land which they sight when coming in from the high seas.]

ABOUT two o'clock in the morning of Friday, October 12, 1492,† Christopher Columbus, who had left Gomera, in the Canaries, steering westward into the unknown Atlantic, on September 6th, and had seen no land since, sighted in the moonlight, from the poop of his flagship the *Santa Maria*, the shores of a low island some miles ahead, and knew that, whatever trials Fate might have in store for him, he had made history, and that the dream of his life was a dream no longer.

The island, which he named San Salvador, and of which, a few hours later, he took possession in the names of Their Most Catholic Majesties, was undoubtedly one of the Bahamas; but its identification with any known member of that group offers a problem which has perplexed geographers ever since, and which cannot even now be said to have been absolutely resolved. In fact, one result of a prolonged controversy has been, I suggest, a gradually strengthening conviction in the minds of geographers that, to speak in mathematical language, a rigorous solution of the problem is impossible; that, as in the case of Euclid's twelfth axiom, the available data,

* After I had read this paper at the R.G.S., a friend present informed me that his wife was under the impression that he was attending a lecture on “The Landslip at Colombo.”

† Old Style. By the Gregorian calendar it becomes October 21.

when scrutinized, implicitly forbid the attainment of such a solution.

If seven cities, so we are told, contended for the honour of being Homer's birthplace (as eleven, certainly, have disputed that of Columbus himself), at least five widely separated islands of the Bahama group have at different times been put forward, and their claims to be regarded as the only true landfall of Columbus strenuously advocated, by various eminent authorities. None of these solutions is free from objection—in one or two cases grave objection—while on the other hand there is something to be said for each. That is, of course, nothing unusual. There is something to be said for any of the score or so of men who have been accused of having written the *Letters of Junius*; just as there was for each of the thirty-odd unfortunate gentlemen who, between 1800 and 1850, claimed to be Louis XVII. The present problem, however, is less complicated than either of these vexed questions—less complicated, indeed, than even the comparatively simple one of identifying Hannibal's Pass over the Alps. I have said that a rigorous solution is impossible, owing to the conflicting nature of the data; but I hope to show that if one accepts the principle of assessing the value of the various solutions by the percentage of evidence which one is compelled to disregard in each particular case, it is not difficult to pick out one solution to which there attaches a very high degree—an outstandingly high degree—of probability.

As I mentioned, the subject has been repeatedly discussed by many eminent geographical authorities; and, as I have no new theory to put forward, I should have hesitated to write upon it if I had not felt that it might at least be useful to compile a summary of the

facts and opinions relative to the problem, and so to make them more readily accessible. It may also be suggested that the past history of the controversy rather indicates that it actually handicaps a writer on the subject to identify himself with any particular theory; most of them have shown themselves far more convincing when demolishing each other's theories than when defending their own—and, with one pronounced exception, their common defect has been, I suggest, a decided lack of impartiality and balanced judgment.

The historical facts in connection with the various identifications of the landfall are briefly as follows:

Beginning from the earliest date at which it can be said that the cartography of the Bahamas was anything more than rudimentary, the island first claimed as the landfall of Columbus appears to be Cat Island. This identification was suggested by Catesby in 1731* and by Knox in 1767,† and held the field unchallenged until 1793, when the learned Spanish historian Munoz put forward the claim of Watling Island.‡

When Navarrete published, in 1825, his classic work upon which all subsequent investigation of the subject has necessarily been based,§ he propounded, for the first time, the theory that Columbus' landfall was Grand Turk Island, and this was supported by his American translator, Kettell, in the Boston edition of 1827, by the Hon. George Gibbs in an article (containing arguments of much greater length than depth) in the *Proceedings* of the New York Historical Society, 1846, and by R. H. Major in the first edition of his *Select Letters of Columbus* (London, 1847).

* *Natural History of Carolina*, 1731.

† *New Collection of Voyages and Travels*, 1767.

‡ *Historia del Nuevo Mundo*, Madrid, 1793.

§ *Coleccion de los Viages* . . . Madrid, 1825.

On the other hand, the Cat Island theory was revived and strongly advocated in Washington Irving's great *Life of Columbus* (avowedly based in large measure upon Navarrete), which appeared in 1828. Irving, with his customary good sense, entrusted the discussion of the landfall to another hand; only referred to, in the first edition, as "an officer of the Navy of the United States" who preferred to remain anonymous—although in the revised edition of 1848 he was revealed as Commander Alexander S. Mackenzie, U.S.N. The claim of Cat Island, in opposition to Navarrete's Grand Turk Island, was also supported, at about the same period, by De La Roquette in the French translation of Navarrete (1828), and by Baron de Montlezun,* while Humboldt† also lent the weight of his European reputation in support of the arguments adduced by Mackenzie.

The claim of Watling Island, which (as already remarked) had originally been put forward by Munoz in 1793, and had since lain dormant, was revived and strenuously advocated by Captain A. B. Becher, R.N., Assistant Hydrographer of the Navy,‡ in an extensive work published in 1856.§ To him is due the presence, on Admiralty charts Nos. 761 and 2579, of a note against Watling Island—"Landfall of Columbus, Oct. 1492"—and also an unfortunate error, to which I will refer presently, in the ascription of two names on adjacent islands.

One of the principal advocates for Grand Turk Island,

* *Nouvelles Annales des Voyages*. . . . Paris, 1828.

† *Examen critique de l'histoire . . . du nouveau continent*, 1837.

‡ This post had not then been officially established, but Becher performed equivalent duties.

§ *The Landfall of Columbus*, London, 1856. Some correspondence between Becher and Gibbs (already referred to as an advocate for Grand Turk Island) appears in the *Nautical Magazine* (founded and, at the time, edited by Becher) for March 1858.

R. H. Major, was converted by Becher's arguments (a rare event), and in the second edition of his *Select Letters of Columbus* (London, 1870) and in the *Geographical Journal* for May 1871, he retracted his former views, and accepted Watling Island as the landfall. Becher was also supported by Oscar Peschel in a work published in 1858.

By this date, then, there were at least three Richmonds in the field—Cat, Grand Turk, and Watling Islands—all with a respectable array of advocates; and there was also a fourth, Mariguana Island, put forward by Varnhagen in 1864.* This theory, so far as I can discover, has never been endorsed by any other authority of standing; and the same remark applies to another, developed with great diligence and wealth of detail by Captain G. V. Fox, U.S.N., in a monograph published in 1882.† He selected Samana Island as the landfall. It should be added that his essay forms probably the most complete and scholarly (although also, I think, the most obviously biased) investigation of the subject extant, and on points of detail it is a perfect encyclopædia of information.

Captain Fox's conclusions were very ably and convincingly criticized by Lieutenant J. B. Murdock, U.S.N., in a paper published in 1884 (after the former's death). This is by very far the most competent and impartial examination of the question which I have been able to discover. Murdock concluded that Watling Island was in all probability the true landfall, but he pointed out grave defects in the reasoning by which Becher had previously arrived at the same conclusion.

The subject was also discussed at length by Sir

* *La Verdadera Guanahani de Colon*, Chile, 1864; Vienna, 1869.

† Report of the Superintendent of the U.S. Coast and Geodetic Survey for the year ending June 1880, Washington, 1882. (Appendix 18, pp. 347-411).

Clements Markham in his *Life of Columbus* (London, 1892). He adopted Murdock's views, giving at the same time a *résumé* of the other theories. His treatment of the question, however, was not altogether satisfactory. As Macaulay might have done, he wrote, apparently, with the sole object of implanting in his readers' minds the same settled conviction as he had formed himself, and his presentation, both of the facts and the arguments, is in consequence extremely one-sided, as well as being inaccurate in many points of detail. In the edition of Columbus' journal which he edited for the Hakluyt Society in the following year he abandoned argument and treated the identity of the landfall with Watling Island as a *res adjudicata*.

In addition to Markham's *Life*, referred to above, the fourth centenary of the landfall saw the appearance of three books of first-class importance in the bibliography of the Admiral—Asensio's monumental biography, Harisse's *Discovery of North America*, and Fiske's *Discovery of America*. Of these the first-named author is in favour of Cat Island, although he prints an appendix in which the case for Watling Island is ably advocated by Juan Ignacio de Armas. Harisse and Fiske content themselves, as do Winsor (1890) and Thacher (1903),* with a summary of the various theories.

It should be added that the navigation of Columbus' first voyage was very ably discussed by the late Earl of Dunraven in an appendix to the second volume of Filson Young's *Columbus* (London, 1906). He did not, however, attempt to identify the landfall, but assumed that it was Watling Island. It may, in fact, be fairly said that

* Thacher refers to an expedition financed by the Chicago *Herald* for the purpose of identifying the landfall. Presumably, so far as that powerful organ's public is concerned, the question is settled.

since the appearance of Becher's work in 1856 the Watling Island theory has gradually made more headway, in the opinion of geographers in general, than any other, and that it is now accepted by the majority, as well as in most standard works of reference. It is, however, of course open for any one to propound a new theory, or to champion an old one, and it will be useful here to take stock of the materials on which such theories must be based, and of the general lines on which the problem can best be attacked.

The original journal of Columbus, and the map which he is believed to have drawn to accompany it, are lost—probably irretrievably. The sole first-hand authority for the voyage which has survived is a lengthy *précis* of the journal, with verbatim extracts, in the handwriting of Bishop Las Casas, the historian of the Indies.* It was examined by Munoz, and published by Navarrete in his *Coleccion de los Viages* . . . previously referred to. Many English translations of the portions relevant to the present inquiry have been made at different times—by Kettell, Becher, Thomas (for Fox), Montaldo (for Murdock—a revision of Thomas's), Markham, and others. In the present essay I have followed Montaldo's version, which, after collating it with several of those instanced, I believe to be the most accurate.

No other contemporary account of the voyage has survived,† and the *précis* published by Navarrete must in

* The date of this MS. cannot be exactly fixed. Las Casas completed his *Historia de las Indias* in 1561, and died in 1566. There is some ground for believing that he worked from a copy of Columbus' journal, and not from the original.

† Some years before the War there were produced in Germany (by Rangette, of Düsseldorf) a number of what purported to be facsimile reproductions of that lost copy of Columbus' journal which he is known to have jettisoned (during a storm off the Azores) when returning to Spain from his first voyage. It is difficult to believe that they could ever have seriously been intended to impose upon any but the half-witted. The text, based on Las Casas, is written

consequence be regarded as the sole real authority for its events. It is therefore very unfortunate (although, in the circumstances of its production, not altogether surprising) that some of its statements, to which I will refer later, are obscure, if not actually unintelligible.

Contemporary first-hand maps, also, are almost entirely wanting. The nearest approach to one is the famous world-map of Juan de la Cosa, a companion of Columbus in his voyage (he was pilot and owner of the *Santa Maria*). It was drawn eight years after the landfall, in 1500. Measuring about 5 feet by 3, it necessarily shows the Bahamas on a small scale, and not very correctly; still, it gives a surprisingly good general idea of the group, and has, I think, been somewhat unfairly criticized. Becher, for example, speaks of it as “. . . an old document that is not worthy to be called a chart. . . .”*

Many other post-Columbian world-maps, of course, are extant, but they are nearly all on such a scale as to be of little value for the identification of “Guanahani,” the native name of Columbus’ landfall.

One map, however, although produced a century later than de la Cosa’s, is of great importance. That is the map of the Bahamas given in Herrera’s *Historia General de las Indias Occidentales* (1601). Herrera was the official Spanish historian of the Indies, and had access to all

in a pseudo-Gothic semi-cursive hand, and in *English* (in my copy), the reason alleged for this being that Columbus had intentionally kept this “secret journal” in a foreign tongue. I believe that this hoax was published simultaneously in several countries, the language employed for the text being varied accordingly.

The book also contained what purported to be a facsimile of Columbus’ commission as Admiral, and was appropriately bound in brown paper, decorated with sea-shells, pebbles, algæ, etc.

* The best reproduction of the Bahaman portion of this chart which I have seen is that given in Harrisse’s *Discovery of North America* (Paris and London, 1892, opp. p. 91). It should, however, be remarked that the accompanying note “enlarged twice the original size” is incorrect. The actual enlargement is trifling—about 10 per cent.

available official documents. It is possible, as I hope to show, to identify Guanahani from this map, and from de la Cosa's, with some certainty.

Apart from the above documents, there is little of fundamental importance in the way of material available, although, of course, much is to be learned from a study of the working out of the various theories and the arguments used for and against them. As regards local information and tradition, little is to be gleaned from this source. The gentle and unsuspicious natives who inhabited the Bahamas at the time of Columbus' arrival were, unfortunately but perhaps excusably, unversed in civilized warfare, and were in consequence rapidly and completely exterminated, leaving the islands swept and garnished for their subsequent exploitation by pirates, bootleggers, hijackers, and other predatory animals. One or two of the native names ascertained by Columbus are still in use, and assist in the identification of some of the islands which he fell in with, but there appears to be no reliable tradition, either in the Bahamas or in Spain, which throws any light on the subject of the landfall. In fact, such tradition as exists has a directly confusing effect—for example, the name "San Salvador" has, since the beginning of the seventeenth century, been applied at various times both to Cat and Watling Islands, and the resulting confusion formed the subject of a well-meant Act passed by the Bahaman legislature in 1926.

No relics, inscriptions, or records which can be definitely associated with Columbus' landfall have been found on any of the Bahamas, nor does he appear to have taken any astronomical observations of its position—in fact, Las Casas's *précis*, as a whole, contains remarkably few of such observations. A small number of observed latitudes (some of which are obviously wrong) are given, but the earliest of

these is dated October 30th. Even if available, Columbus' longitude of Guanahani would be practically valueless as a means of identification, both by reason of his primitive methods and of his well-known underestimate of the length of a degree; but an observed latitude of the landfall would have been of considerable assistance.

The methods which, at first sight, would appear to be available for investigating the question of the landfall are as follows:

1. By working up the courses and distances sailed by Columbus between Guanahani and his last-known point of departure—Gomera.
2. By using the direct evidence afforded by the charts of de la Cosa and Herrera.
3. By comparing the description of Guanahani which Columbus gives with that of the various likely islands as they exist to-day.
4. By constructing, from the data given in the journal, a plotting of the relative bearings and distances of the various islands discovered by Columbus, and fitting this in on a modern chart.

Before applying any of these methods, the following notes upon one or two general points may be useful.

The courses, bearings, distances, and dimensions given in Columbus' journal are, of course, only approximate. One could hardly expect them to be otherwise. He had no hand-log, and his dead-reckoning must therefore, even when unfalsified, have been quite sketchy, while the distances and dimensions of the islands which he sighted are probably no better than rough eye-estimates. The allowance, if any, which he made for leeway (which in the ships of his time was very considerable) is nowhere mentioned. His courses and bearings appear to have been

given to the nearest two points; at least, while he occasionally logs intermediate points such as N.N.W., he does not employ by-points. His distances are generally given in round figures, and mostly in leagues. Captain Fox, in the monograph previously referred to, made a careful investigation of the probable values of Columbus' "league" and "mile," and reached the conclusion, which appears well founded, that no great error would be occasioned in calling the "league" 3 nautical miles, and the "mile" three-quarters of the present nautical mile. He also published in the same work a very careful investigation by C. A. Schott (U.S. Coast and Geodetic Survey) of the probable amount of variation obtaining in the Bahamas in 1492. This showed that, in all likelihood, such did not exceed a quarter of a point westerly.* In view of the approximate nature of Columbus' bearings and distances, therefore, it appears best to treat them as being "true," and to convert all quoted distances to nautical miles. While on the subject of distances, it is interesting to note that Columbus frequently speaks of short distances as "two lombard shots," or "two cross-bow shots." The lombard was a species of culverin—a smooth-bore cannon of small calibre and short range. I have not succeeded in obtaining conclusive evidence of its carrying power, but there would probably be no great error in regarding a "lombard shot" as two cables† and a "cross-bow shot" as half a cable.

The first method, that of working up Columbus' run from Gomera to the landfall, is impracticable. The great circle run from Gomera to the nearest point of the Bahamas is about 3,100 miles, and at this distance the whole

* Becher found it necessary, in support of his views, to assume that the variation was a point and a half westerly, but there seems to be absolutely no justification for this assumption. A "point" is $11\frac{1}{4}^{\circ}$.

† 400 yards.

chain of the Bahamas, from Bahama in the north-west to Grand Turk in the south-east, would subtend an angle of only about 11° ; in other words, even if one could depend upon having worked out Columbus' "course made good" to within half a point, which is a much closer degree of approximation than one could reasonably expect from such data, one would still have all the Bahamas to choose from, while to identify any particular island with certainty one would have to be able to depend upon the "course made good" being correct to about a quarter of a degree—a total impossibility.

The second method—that of using the direct evidence afforded by the charts of de la Cosa and Herrera—is more promising, although not altogether conclusive in the results which it affords. As already stated, while de la Cosa's chart is, for its date, surprisingly accurate in the general idea which it affords of the Bahama group, it is woefully inaccurate in detail. Broadly speaking, it indicates that Guanahani is a comparatively small island, standing out a little to the north-east from the general north-west-south-east line of the Bahamas and towards the centre of that line, approximately due northward of the strait dividing Cuba from San Domingo, and due eastward of the north-west point of Cuba.

On Herrera's chart, which supports this inference, the Bahamas are indicated with more precision, while in addition all the islands at present known to exist are shown (de la Cosa's chart is defective as regards the north-west islands). The topography of Herrera's chart is markedly inaccurate, but the *relative* shapes and sizes of his islands are not far from the truth, although their general trend, considered as a whole, has an anti-clockwise error of about 15° .

If we accept the evidence given by the charts of de la

Cosa and Herrera, which is in essential concordance, there are *prima facie* grounds for immediately non-suiting two claimants out of our "Big Five"—Cat, Watling, Grand Turk, Samana, and Mariguana Islands. For on both charts we see Samana, Mariguana, and Guanahani shown as three entirely separate and distinct islands. It has, indeed, been argued at length by Fox that the name "Samana" was, at the date of Columbus' discovery and for some time after, applied to one of the Crooked Islands, while the present Samana was Guanahani. His arguments are ingenious, but little more. On the other hand, the position given for Samana by Herrera is admittedly very incorrect—much more so, relatively speaking, than that of any other island on his map.

Both charts, moreover, combine against the claim of Grand Turk. If this were accepted, we should have to assume that all the islands shown by both de la Cosa and Herrera to the south-eastward of Guanahani were figments of their imaginations—an assumption which a glance at the modern chart will show to be untenable. A reasonable deduction, it is suggested, from the evidence of the charts is that they show only two admissible hypotheses—namely, that Guanahani must have been either Cat Island or Watling Island, the probabilities being greatly in favour of the latter.

I turn now to the third method—that of attempting to identify Guanahani from the description of its topography, etc., recorded by Columbus. That description, briefly, is as follows. It was an island of moderate size, low, well wooded, with abundance of water and fruit, having no high hills, with a very large lagoon in its middle, and encircled by a reef. One side trended to the north-north-east, and Columbus especially noted a peninsula which, as he remarked, could easily be fortified.

The Bahamas, which have a total area of about 4,400 square miles, and contain some 700 islands and cays,* may be thought to offer a tolerably wide field to any one who, on the evidence of this somewhat vague description alone, should set out to identify the island to which it refers. But, as I hope to make plain later, any one who puts forward a considered theory as to the location of Columbus' landfall is morally bound, or so it appears to me, not only to select an island corresponding with this description, but also one from which Columbus' track—so far as this can be made out from the fragments of his journal preserved by Las Casas—and the islands and cays discovered along that track, can be brought into reasonable agreement with the topography of the Bahamas as at present known. This condition practically reduces the field of inquiry to the "Big Five" of which I have already spoken.

I take this opportunity of referring to a suggestion which has several times been mooted; namely, that our troubles in identifying the landfall of Columbus possibly arise from volcanic or other extensive changes which have taken place in the Bahamas at some date between 1492 and the beginning of the nineteenth century, by which time their topography (chiefly as the result of work done by H.M. Surveying vessels) had emerged from the comparative twilight of "sketch surveys" and other dangers to navigation, and was known in all essentials as it is known to-day. Such a theory is, of course, impossible to disprove satisfactorily, or even to dismiss as highly improbable: the calamity of Martinique in 1902 goes to show that volcanic disturbances on an

* These figures are taken from the recent *Maps of the Bahamas: published by authority*, 1926. This work states that "the island officially known as San Salvador is generally called Watlings (*read* Watling), and is now accepted as the island called San Salvador by Columbus."

almost unparalleled scale have before now given the West Indies a dreadful eminence in the history of seismology; but at the same time we should, I suggest, follow Newton in saying *hypotheses non fingo*. If a matter can be cleared up without the aid of suppositions which, however possible, are highly improbable, we have no right, on an ordinary estimate of probabilities, to invoke them. The volcanic changes which would destroy the general outlines of the topography of the Bahamas, or which would, in other words, allow us to "remould it nearer to our heart's desire" in respect of some particular theory, would connote an alteration in general level of many yards—and in support of such a sweeping and Wegener-like assumption there is, so far as I am aware, no evidence whatever.

Let us, therefore, turn to the question of comparing the description of Guanahani given by Columbus with that of the "Big Five" as contained in the Admiralty Sailing Directions and other standard sources of information.

Cat Island is roughly 40 miles long north-west and south-east by about 10 miles wide at its south-east end. Its maximum height is about 400 feet.* At the south-east end (the end likely to have been examined by Columbus) are several lagoons. It has no fringing reef (it should, however, be noted that such a reef would in all probability have suffered very extensive changes between 1492 and, say, 1830) and no pronounced portion of coast-line trending north-north-east.

Grand Turk is low and has large lagoons and an

* Markham, in his *Columbus*, p. 95, remarks: "Guanahani was low; Cat Island is the loftiest in the Bahamas." It is undoubtedly true that Cat Island is slightly higher than its neighbours, but quite imperceptibly so. All alike are very low-lying, and "lofty" seems a somewhat misleading term to apply to an island 40 miles long and nowhere rising more than 400 feet above sea-level.

encircling reef. On the other hand, it is exceedingly small (only about 2 miles long by 1 mile in breadth), no portion of its coast-line runs for any distance north-north-east, and it has no woods, and no peninsula.

Watling Island is 13 miles long by about 6 broad. It has a very large lagoon at its centre, and is extensively wooded. The greater portion of its eastern shore runs north-north-east and south-south-west, and it is low, nowhere exceeding 140 feet. It is also encircled by a reef. On its eastern side is a narrow peninsula, serving as a natural breakwater to a moderate-sized harbour.

Mariguana Island is 23 miles long, east to west, and from 2 to 6 miles broad. It has no real lagoon, but a few coastal ponds, the largest about 5 miles in length and, as they all are, very narrow. It is low-lying, nowhere exceeding 100 feet. Its south-east extremity has a short coast-line, some 5 miles in length, running about north-east and south-west. It has no fringing reef; on the other hand, there is a well-defined peninsula half-way along its northern coast.

Samana Island (also called Samana Cay and/or Atwood's Cay) is about $8\frac{1}{2}$ miles long by $1\frac{1}{2}$ miles broad. It runs practically east and west, and has no eastern side at all. It has no lagoon or fringing reef, and no peninsula.

Let it be noted that the absence of all or any of the "stigmata" is not necessarily a conclusive proof or disproof of a particular claim.* Much may have happened in four centuries: a lagoon may have formed or filled up; erosion may have converted a peninsula into an island, or accretion rendered it an indistinguishable part of the neighbouring coast-line; a belt of timber may have widened or withered.

* So far as one important feature, the lagoon, is concerned, there is another island, Great Inagua (21° N., $73\frac{1}{2}^{\circ}$ W.), which can compete with any of the "Big Five." I do not, however, envy the lot of any one who endeavours to work out a consistent track for Columbus from Great Inagua to Cuba.

At the same time, since by the rules of probability it is unlikely that all these causes should have operated in the same direction in any one case, it is to be inferred that the fairest estimate of the relative merits of the five claims is to be arrived at by comparing the present facts with Columbus' description without making any allowance for subsequent changes, whose amount and extent it is quite impossible to ascertain or even to estimate.

If we do this, the account will run as follows: Watling Island complies with all the requirements; after it, in order of minimum rejection, come Mariguana, Cat Island, Grand Turk, and Samana.

Finally, we come to consider the information, with regard to the location of the landfall, afforded by the descriptions which have survived (in Las Casas' *précis*) of the relative sizes, shapes, distances, and bearings of the various islands seen by Columbus.

After leaving the landfall the Admiral—who had, he records, made up his mind to pass no island without taking possession of it—visited in turn three other islands, and then, after sighting a group of small islets, fell in with a fifth island, much greater in size than any of the remainder. This island was undoubtedly Cuba.

There is sufficient information available in Las Casas' abstract of the journal to construct a rough charting of these islands, but certain assumptions are necessary, and there are one or two defects of importance. The fifth island, as I remarked above, was undoubtedly Cuba, and the fourth and third can be identified with a fair amount of certainty. The second is more doubtful; and, unfortunately, although we are given the distance of the second from the first—the landfall—we are not given its bearing.

It has been generally assumed, even by the usually

very careful Murdock, that the second island bore south-west from the landfall, this assumption being based on the fact that Columbus stated (in his journal for Saturday, October 13th) that he intended to sail, on the following evening, south-west in search of other islands. But as evidence to his actual course this is entirely negatived by a statement, in the journal for the following day, that having set sail he saw so many islands that he did not know which to make for first, but that he finally selected the largest.*

Actually this statement is very obscure, for it does not seem possible that from any position in the neighbourhood of his probable landfall he could have seen any such archipelago as he describes. Possibly, but not probably, he was misled by clouds hanging over islands, not actually in sight, below his horizon. But in any event, whatever its value, I submit that it entirely inhibits us from assuming that he necessarily steered south-west to the second island.

Let me illustrate. Suppose that a friend told you that he was going next day to the Motor Show, and that he intended to buy a car of a certain make. If you simply heard later that he now owned a car, you would naturally conclude, on that evidence, that it was of the make he had mentioned. But supposing that he wrote to you and said that when he got to Olympia he saw so many cars that he couldn't make up his mind at first, but finally selected a car which suited him more than any other, you would then be left in complete uncertainty as to what particular make of car it was. I apologize for labouring the point, but it is of some importance, and does not seem to have been fully appreciated in the past.

* His masthead height may have been 60 feet. From this elevation Cat Island (400 feet) would have been visible some 30 miles,

Summarizing all the evidence contained in the journal as to the size, bearings, distances, etc., of the five islands, it amounts roughly to the following:

The second island ("Santa Maria de la Conception") lay about 18 miles from the first island ("San Salvador" or "Guanahani")—bearing not stated. The side towards Guanahani ran north and south 15 miles, the other east and west 30 miles. It was probably in sight from a point somewhere near Guanahani, but not from that island itself.

The third island ("Fernandina") was in sight from the second, and about 24 miles west of it. It extended about 75 miles or more in a direction roughly N. 25° W. to S. 35° E. On the east side was an inviting-looking harbour which was found to be shallow. This had a narrow entrance, with a rocky islet on the east side of its centre.

The fourth island ("Isabela") was not in sight from the third. Off its northern shore was a rocky islet, bearing east from the third island; distance not stated, but about four hours' run with a fair wind. From this islet the coast ran south-south-west* to a prominent cape. The native name of the island was "Saometo" or "Samaot." It was even more beautifully wooded than the other islands, and somewhat higher, but by no means mountainous. It was suspected to consist of two closely contiguous islands, and had large lagoons.

The group of islets ("Islas de Arena") formed a chain extending in a north and south direction. On their south side was shallow water for some 15 miles. They were situated roughly 70 miles west of the west side of the fourth island.

* This bearing is partly inferred. At another place in the journal it is given as west 35 miles, which would make the cape in sight from the third island.

The fifth island ("Juana") was sighted after running about 70 miles south-south-west from an anchorage to the southward of the group of islets.

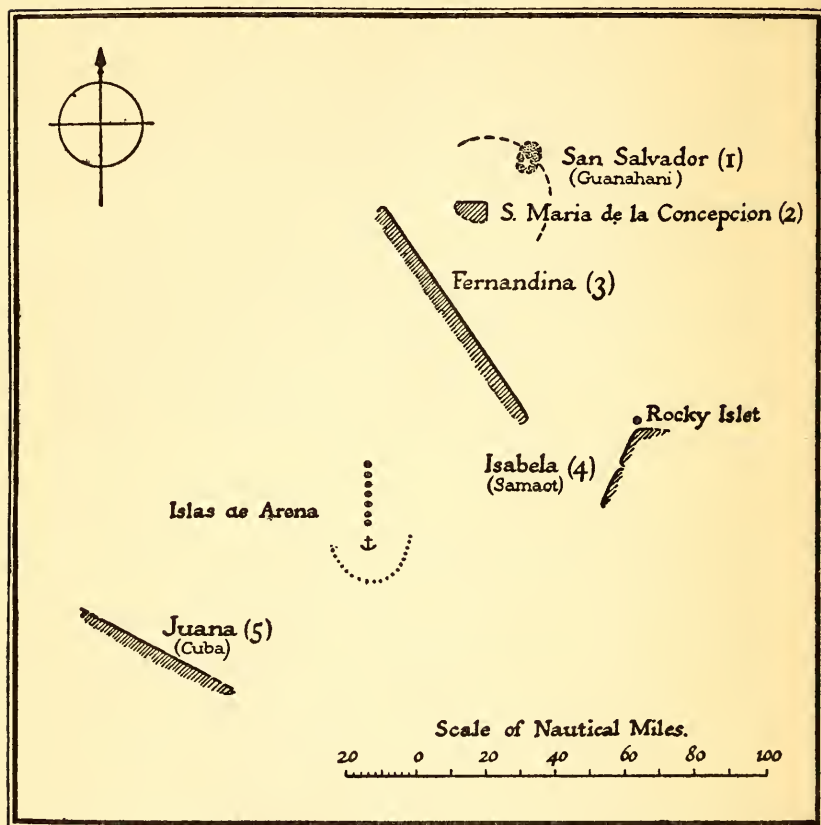


FIG. 3.—Chart of the islands sighted by Columbus, drawn from the data contained in his journal. (See also Fig. 10.)

It is possible, with these materials, to put together a very rough sketch-chart* which should give some idea of what Columbus' own map, if he ever drew one, would have resembled. In so doing, strictly speaking, where the

* See Fig. 3.

bearing of one island from another is given with no indication of the particular portion of the island to which the bearing refers, one should put the centre of the island on the bearing. But as, in the present case, both the second and fourth islands are stated to lie about the same distance eastward of the third (a long island running about north-north-west and south-south-east) and not to have been in sight from each other, it seems a fair assumption that the second lay somewhere off the north-north-west end of the third and the fourth somewhere off the south-south-east end.

It should be noted that this sketch-chart has been put together, almost entirely, by means of the relative bearings and distances of the various islands one from another, as given in the journal; and that no use has been made, except where absolutely necessary, of the courses and distances logged by Columbus' ships, which are vitiated by all sorts of errors—such as over- or under-estimation of speed, errors of steering, lee-way, current, etc.—uncertain both in amount and in direction.

It is not, of course, claimed that so rough a chart can give more than an indication of the relative configuration of the islands visited by Columbus, but it will, I think, be found amply sufficient to assist in testing the comparative merits of the six complete tracks (from the landfall to Cuba) which have been put forward by different authorities; and such a test is, I suggest, the most searching and the most efficient to which any theory of the landfall can be subjected.

Taking these tracks in chronological order, the first is that of Navarrete, who selected Grand Turk as the landfall, and who also assumed, on no authority that I have been able to discover, that Columbus always

sailed towards the westward in his courses from the landfall to Cuba.

Navarrete makes the Caicos group the second island, Little Inagua the third, Great Inagua the fourth, and Cuba the fifth (all the tracks agree upon this last point). The courses and distances which Columbus must have traversed to follow such a route are entirely at variance with anything to be found in the journal, as also are the



FIG. 4 — *Navarrete*, 1824.

dimensions of the various islands; for example, the selected third island, Little Inagua, is only $7\frac{1}{2}$ miles long, instead of Columbus' 75 or so. The "Islas de Arena" are not identified but simply omitted.* As Murdock severely but justly remarks, ". . . It is hardly possible to imagine that this track is derived from the log at all."

Mackenzie starts from Cat Island, which he assumed to be the landfall; and it should here be noted that there is

* Murdock states that Navarrete omitted the fourth island, as well as the "Islas de Arena," but this is incorrect. See above.

one argument in favour of this theory which applies to it exclusively.* That is, that it is the only one which attempts to explain what, following Dr. Watson (the celebrated and Bæotian companion of Sherlock Holmes) might well be called “the singular incident of the light in the night-time”—an incident which has always been one of the standing puzzles in connection with this subject. About 10 p.m. on the night of October 11—i.e.



FIG. 5.—*Mackenzie 1828.*

about four hours before making the landfall and an hour before moonrise—Columbus and others with him on the poop of the *Santa Maria* saw, or thought they saw, a light some distance away in the darkness. It is described as looking like the flame of a small candle, alternately raised and lowered. It seems to have gone out of sight again not long after.

* So far as the “Big Five” are concerned. The selection of Conception Cay (Conception I.) as the landfall would also allow of our supposing that the light was on Watling I.

Judging by the speed of the ships, as given in the journal for this night, the light must have been some 35 miles or so eastward of the landfall, and well to windward of it. Markham considers that it must have been in a native canoe, but does not suggest any reason why such a craft should have made so laborious and purposeless an excursion out into the Atlantic, away from the Bahamas and practically dead to windward. Nor does it seem likely that a native canoe would have had much use for a light, or have carried the appliances for making and exhibiting it.

Murdock cuts the Gordian knot by suggesting that the light was non-existent—a figment of excited imaginations. This theory of collective hallucination is certainly possible, but it seems rather a drastic way of treating inconvenient evidence. Mackenzie, on the other hand, supposes that the light was on Watling Island; and that Columbus, owing to the darkness, missed that island, although seeing the light, and passed on to make his landfall at Cat Island. This theory, taken by itself, seems reasonable, as the run between light and landfall would then fit in quite well. But it is, on any supposition, very surprising that if Columbus (as he seems to have done) looked on the light as a sure indication of the long-expected land, he should have calmly proceeded under all plain sail and let it go out of sight astern, instead of clewing up and waiting for daylight, or even for moonrise.

From Cat Island Mackenzie sends Columbus to Conception Cay, which he selects as the second island, making Great Exuma the third, and Long Island the fourth; after which he dispatches him south-westward to Cuba, straight across the Great Bahama Bank. This portion of the route (and also that between Exuma and

Long Island) is, and probably always has been, practically unnavigable.

In addition to this fatal defect in Mackenzie's track, his second island is far too small, and his third island is not in sight from it. Moreover, he makes Columbus go from Conception Cay to Exuma without apparently noticing Long Island, although we are told in the journal that he resolved to pass no island without taking possession of it.

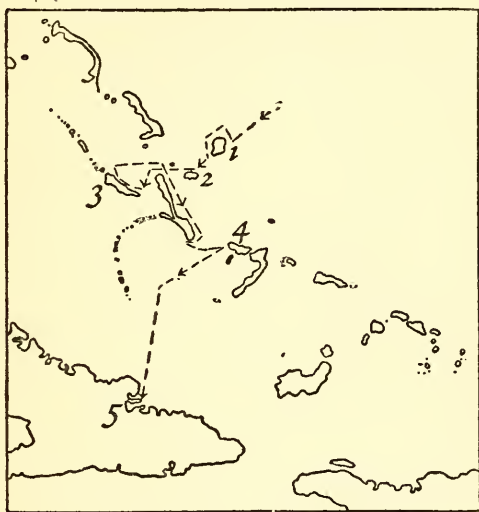


FIG. 6.—Becher, 1856.

Becher, having fixed on Watling Island as the landfall, makes Rum Cay the second island. As explained, the latter's bearing from the landfall is uncertain, while as regards distance Rum Cay fills the bill more or less satisfactorily. Columbus, however, describes the second island as about 5 *leagues* north and south by 10 east and west, while Rum Cay extends only about those numbers of *miles* in such directions. Becher neglected this point; but it does not seem to be a fatal objection. It was later shown by Murdock, and I think conclusively, that Rum Cay is a

more generally satisfactory "second island" than any other yet proposed, and it may be noted that its dimensions agree in relative proportion, although not in actual amount, with those given in the journal. It is possible that Las Casas, or the copyist, simply wrote "leagues" for "miles,"* which would explain the whole difficulty; but one hesitates to make such assumptions, since it is fatally easy, in this manner, to twist facts to suit theories.

But Becher, having gone so far, proceeds to go definitely and demonstrably wrong in his further discussion of the track. Mistranslating an expression in the journal, ". . . cargué las velas . . ." ("clewed up the sails") as ". . . I made sail . . ." he sends Columbus, in flat defiance of the journal, past the second island, Rum Cay, without stopping, and thence to the third island, which, like Mackenzie, he identifies as Great Exuma. He then, to the confusion of common sense and, one would think, in utter forgetfulness of his own very considerable nautical experience, assumes that during the night of October 16-17 Columbus, in a gale and off an unknown coast, ran 100 miles in ten hours along the east coast of Long Island without noticing that it was not part of Exuma, and so arrived on the following day at Crooked Island, which Becher selects as the fourth island. Putting aside the fact that no sane man would navigate in so utterly reckless a manner, it need merely be pointed out that the gale in question only existed in Becher's imagination, the journal for that night recording ". . . the wind was light and did not allow me to reach the land to anchor. . . ." As Fox rather caustically suggests, Becher appears to have confused Columbus with Vanderdecken.

* Markham, having mooted this assumption in his *Columbus*, states it as a simple fact in his translation of the journal.

From Crooked Island Becher makes Columbus anchor south of the Ragged Islands, which he identifies with the "Islas de Arena," and then lets him go on his way, without further perilous adventures, to Cuba.

A curious error, arising directly out of Becher's resolve not to let Columbus stop at the second island, still appears on the Admiralty charts. The name of that island, "Santa Maria de la Concepcion," is divided,



FIG. 7.—*Varnhagen*, 1864.

one-half appearing as C. Santa Maria (Long Island), and the other half as Concepcion Island!

Varnhagen, assuming Mariguana as the landfall, makes the second island Acklin Island. Regarding Acklin and Crooked Islands as one (they are closely contiguous), this would agree fairly well with the dimensions given in the journal, although not at all with the distance between the first and second islands. He fixes on Long Island for the third island, and makes Columbus circumnavigate it—after which he brings him back to Crooked Island,

which he must have passed on his way to Long Island, and makes him regard it as an entirely new discovery—the fourth island! Like Becher, he identifies the “Islas de Arena” with the Ragged Island chain.

Fox, although selecting Samana as his Guanahani in place of Mariguana, is otherwise in substantial agreement with Varnhagen. Like him, he goes to the Crooked Island group for the second island, although he selects



FIG. 8.—Fox, 1882.

Crooked Island itself, and not Acklin. He, too, makes Long Island the third island, and brings Columbus back afterwards to the Crooked Island group for the fourth island, this time selecting Fortune Island (close south-west of Crooked Island). He agrees with Varnhagen (and with Becher) as to the identity of the Ragged Island group with the “Islas de Arena.”

From the text of the journal itself, and on such grounds alone, the plan, followed by Varnhagen and Fox, of making Columbus sail half-way round the Crooked Island

group, proceed to Long Island, and return again to the Crooked Island group under the impression that it was an entirely new and different "fourth island," out of sight of the "second island"—i.e. practically, of itself—seems entirely untenable, and remarkable only as an excellent example of the extent to which very able and honest men can, under the influence of theories, shut their eyes to facts. It seems scarcely necessary to "flog the dead horse" any further, but it may be pointed out that Long Island, for instance, is not visible from any part of the Crooked Island group, whereas Columbus most distinctly stated that the third island was in sight from the second.

The track worked out by Murdock in 1883 differs greatly from its predecessors—not only in its configuration but in its superior plausibility, and also in the method by which it was obtained. Instead of starting with some island assumed as the landfall and then constructing a track from this to Cuba, Murdock reversed this plan, and worked backwards from Cuba until he reached the landfall.*

At the outset of applying this method, however, there appears to be an almost insuperable difficulty. Although the fifth island of Columbus can only—in view of its size, the trend of its shores, the corroborative evidence of Columbus' successors and his own later voyages, and the persistence of its native name from then till now—be identified with Cuba, it is not so easy to locate the part of it which Columbus first sighted and the harbour in which he first anchored. There are at least four candidates for the latter—Port Nipe, Port Gibura, Port Padre, and the Boca de Caravela. The only real clue is a statement

* This is undoubtedly the more scientific method. It is, however, curious to note that Caleb Cushing, in an article in the *North American Review*, remarks that after applying it "we . . . shall be convinced that Guanahani is no other than Turk's Island"!

in the journal that the harbour had a wide entrance with 12 fathoms of water. None of the four exactly corresponds with this. Murdock, with considerable probability, selects Port Gibura.

It might be thought, then, that whether you work out the track forwards or backwards you have to assume a starting-point. But, as Murdock showed, this can be avoided in working from Cuba to the landfall. The first



FIG. 9.—Murdock, 1884.

point to identify from Cuba is the anchorage at the “Islas de Arena,” which, from the journal, is found to have been about 75 miles north-east from the harbour in Cuba. If, therefore, one takes the outline of the north-east coast of Cuba and transfers it bodily 75 miles north-east, this line should pass through or near the anchorage south of the “Islas de Arena,” and a short examination shows that these can only have been the Ragged Islands.

Having determined this point, it is similarly possible to work backwards to the position, off the fourth island,

from which Columbus took his departure for the "Islas de Arena." This is given in the journal as being south-east 21 miles from the south point of the third island, and about west-south-west from the rocky islet at the north end of the fourth island, while it was about 65 miles east $\frac{1}{2}$ north from the "Islas de Arena" (which have just been identified as the Ragged Islands). These data point with practical certainty to Long Island as the third island and Crooked Island as the fourth,* the rocky islet being Bird Rock off Crooked Island, which agrees exactly, both in position and appearance, with the account given in the journal. The harbour on the east side of the third island was probably Clarence Harbour, Long Island.

So far then, working backwards from Cuba, the "Islas de Arena" and the fourth and third islands have been identified with very fair certainty. It remains to identify the second island, and thence, if possible, the landfall. And it is here the real difficulties begin.

As previously suggested, since the third and the second islands are both stated to be situated about the same distance—25 miles or so—eastward of the third, and out of sight of each other, it is a reasonable assumption that the third lay off its south-east extremity, and the second off its north-west. If this is accepted, the second island almost selects itself—it must be Rum Cay; there is no other available. On the other hand, there are two objections to this identification. One is, that the distance to Long Island is smaller than Columbus states, being only 15 miles instead of 21 or so; and the other is the discrepancy in size, although not in shape, already mentioned. The distance, however, was probably overestimated by

* It may be noted that the native name of Crooked Island is "Samoete": compare the name "Samaot" recorded in the journal as being given by the natives to the fourth island.

Columbus; in fact, it must have been, for if it had really been 21 miles the third island could not have been seen from the second unless it were far higher than any land now known to exist in the Bahamas. Murdock considers that the fact that Rum Cay is the only possible "second island" in sight from Long Island must be set off against the discrepancy in distance; but I suggest that he might with more reason have argued that this discrepancy was in fact unimportant, and that the only difficulty was the discrepancy in size. On the whole I think that, reviewing and balancing all the data, there cannot be much doubt that Rum Cay has a pre-eminent claim to be regarded as the second island, although it must be admitted that the identification is not so convincing as in the cases of the islands sighted subsequently.

So far I have, in essentials, simply followed and endorsed Murdock's argument. But, for the reasons I have already given, I cannot altogether assent to his final step. Having reached Rum Cay, he says in effect this: "The second island lay about 21 miles south-west from the landfall. Rum Cay is the second island, and it lies about 19 miles south-west $\frac{1}{4}$ south from Watling Island. Therefore Watling Island must be the landfall."

The correct conclusion, I suggest, should be this: "Rum Cay is, in all probability, the second island. That island, we are told, lay about 18 miles from the landfall, bearing not stated. The landfall must be an island agreeing with Columbus' description and lying on or near a circle of 18 miles' radius drawn from Rum Cay."

Actually, it makes little difference. If we draw such a circle, there are but two islands on or near it. Watling Island is one of them, Conception Cay the other. Watling Island agrees in every respect with the account in the

journal—Conception Cay does not.* Cat Island, whose southern extremity does accord fairly well with the description, and which alone of the “Big Five” claims to explain the mysterious light, is a long way outside the circle—its minimum distance from Rum Cay is 34 miles—and there is no other possible claimant; at least, no other now rising above the surface of the Atlantic.

I ought in conclusion to indicate a question, in connection with the identification of Guanahani, to which it does not seem likely that any satisfactory answer will ever be given. I have already referred to two difficult points—the light, and the archipelago said to have been seen by Columbus immediately on sailing from Guanahani; and I might mention that I once thought I had evolved a theory which would cover both of them. I should explain that I had then been studying the question for fully six hours, and was, in consequence, quite convinced that I knew all about it. I thought that by selecting the south-west point of Cat Island as my landfall (Mackenzie took the south-east point) and assuming that Columbus, on weighing, stood westward for some distance across Exuma Sound, he would then have sighted the string of cays stretching north-west from Great Exuma, while the light, as Mackenzie suggested, would have been on Watling Island. Unfortunately, further consideration knocked the bottom out of this theory altogether. For it to hold good Great Exuma must have been the second island, with whose description this does not agree to any noticeable extent, while all around the corresponding position in

* It is very small— $2\frac{3}{4}$ by $1\frac{3}{4}$ miles—grows only stunted vegetation, and has a conspicuous detached islet on its east side which Columbus would certainly have mentioned. On the other hand, it possesses a lagoon and a fringing reef, part of its coast runs north-north-east, and, equally with Cat I., its selection as the landfall would explain the mysterious light.

which the third island ought to be there is, unfortunately, a painfully aching void.

The difficulty to which I am now referring, however, offers no loophole for such off-hand speculations. It arises from an entry in Columbus' journal about a month later (November 20, 1492). He there states that, being in a position east-south-east of Babeque, he was at the same time 12 leagues from Isabela (the fourth island), adding the exasperating note that Isabela is 8 leagues from Guanahani, or San Salvador. It seems quite impossible to reconcile the courses, distances, and bearings given in the journal between October 12th and 20th with the statement that the landfall and the fourth island, or any part of them, are only some 24 miles apart. The minimum distance between Watling Island and Crooked Island is about 67 miles. We must, I think, take refuge in the last resort of the baffled investigator, and declare that the passage in question is corrupt.

It may be noted that while Columbus himself never revisited the Bahamas, and while they were not much explored by his immediate successors, there is a passage in the account of Ponce de Leon's first voyage to Florida in 1512 which seems, at first sight, likely to throw a good deal of light on the identity of Guanahani. Unfortunately, it flatters only to deceive. According to Herrera, de Leon on his sixth day out from Porto Rico arrived at Caicos, in the Bahamas, and on the eighth at an island named Yaguna, in lat. 24° N. From this he went to another, named Manuega, in lat. $24^{\circ} 30'$, and on the eleventh day arrived at Guanahani, in lat. $25^{\circ} 40'$ N., which, it is added, was the same island as that first discovered by Columbus, and by him named San Salvador.

Whatever the actual error of these latitudes might be (and it is probably considerable) we could, assuming that

it was fairly constant for all, use them to identify Guanahani if we were certain of Yaguna and Manuega; or, conversely, we could, if we were certain of Guanahani, identify the other two. But we are not certain of any of them, and it is fatally easy, in hunting round for Yaguna and Manuega, to fit these names on to some promiscuous pair of islands which agrees with that theory of the identity of Guanahani which one happens to hold. Both Humboldt and Fox certainly appear to have done so. I am afraid that this itinerary of de Leon's is not of much assistance.

But I suggest that while we may never, in the absence of Columbus' full journal and/or map, have absolutely irrefutable evidence as to the identity of Guanahani, we have at least enough to support a strong and reasonable conviction. Like many other celebrated historical mysteries, research has narrowed the issue down to a comparatively small compass. Just as it is fairly certain that the Man in the Iron Mask was either Mattioli or Eustache Dauger; just as it is long odds on Sir Philip Francis for the Junius Stakes against any other nominee; so, I suggest, is it reasonable to take the view that nearly two centuries of discussion and research have by no means been barren of result—that they have ventilated the question pretty thoroughly and shown that, although the data that we possess embody so many and to some extent so conflicting requirements that it is highly improbable and probably impossible that any one island will ever be found to satisfy them entirely, there is one which stands head and shoulders above the others in the number which it satisfies and the fewness of those which its selection compels us to reject. And that island is Watling Island;* or, if we must give it its new legal title,

* See Fig. 10.

(San Salvador or) Watlings Island. Whether we go by the evidence of the charts of de la Cosa and Herrera, by that of the description of the landfall given by Columbus himself or by that of the track which he followed from the

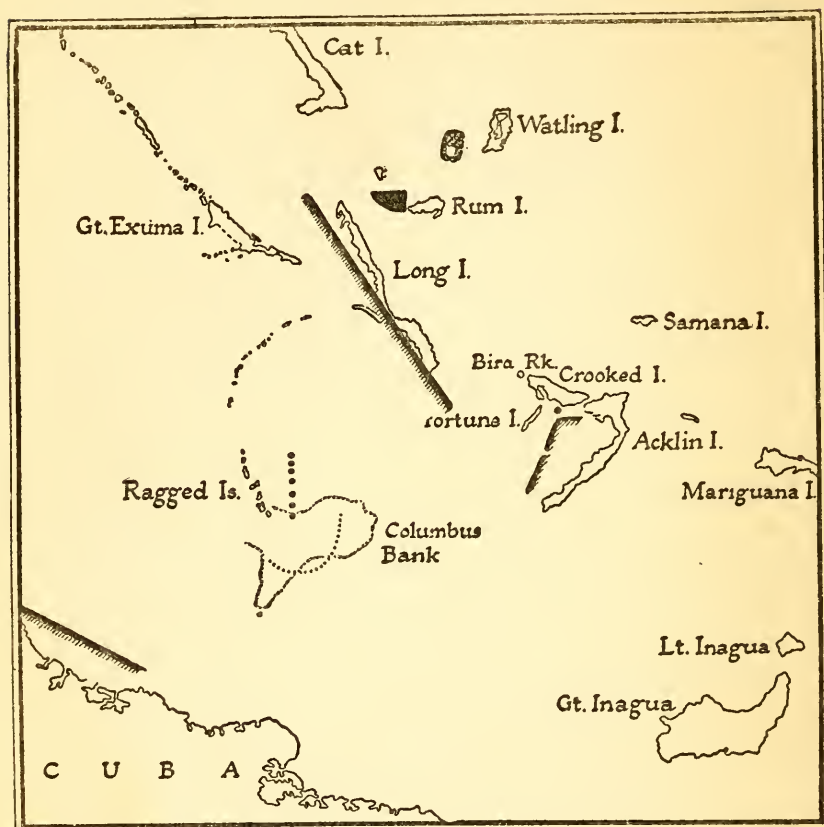


FIG. 10.—Columbus' chart, as reconstructed (see Fig. 3), superposed upon a modern chart of the Bahamas.

landfall to Cuba—and on whatever system we “weight” the results of these examinations—we are, I submit, independently led in each case to the same conclusion, while the cumulative effect of this triple agreement is almost irresistible. Eclipse is first, and the rest nowhere.

In rejecting, if we decide to reject, the claims of Cat Island (which, next to Conception Island, I should class as *proxime accessit*), Mariguana, Samana, and Grand Turk, we imply no censure upon their advocates—rather, I suggest, do we actually honour such men's labours, all of which have, indirectly, contributed to at least a provisional solution of this famous problem.

It is invidious to make a selection, but it is difficult in such a matter to avoid having a preference, and I should like to take this opportunity of putting it on record, as my personal opinion, that it is to Lieutenant (now Rear-Admiral) J. B. Murdock, U.S.N., more than to any other man, that the world owes the first clear and impartial statement, unshaken in essentials by later criticism, of the reasons which go to show that in all probability Watling Island is the true Guanahani—the San Salvador of Columbus, and the spot where, on that eventful morning in 1492, our half of the world began, for the first time, to know *where* the other half lives. *Palmarum qui meruit ferat.*

BEALINGS BELLS

ON March 1, 1834, there appeared in the *Ipswich Journal* a long and remarkable letter. It was from Major Edward Moor,* F.R.S., and described the extraordinary disturbances which had occurred, and were still occurring, in his house at Great Bealings, Suffolk.

An occasional "runaway ring" is not very uncommon, especially when small boys are about; but at Great Bealings peals of four or five house-bells would often be rung simultaneously (and with extraordinary violence) while a watch was actually being kept on them, and when it seemed absolutely out of the question that this could have been done by any human agency. These peals, and occasional single rings, continued to occur at frequent and irregular intervals during a period of some seven weeks—and, in spite of repeated efforts, no definite cause for them could be discovered.

The *Ipswich Journal*, not unnaturally, opened its (fortnightly) columns to further correspondence on the subject; and to this Moor (whose good faith was unquestionable) contributed, from time to time, further bulletins as to the progress of the disturbances. And when these had finally stopped, and the matter had become a mere nine days' wonder, he devoted his leisure (of which he

* A well-known writer on Hindoo mythology. He was born in 1771, and served in the East India Company's forces from 1782 to 1806, when he retired, with a special pension for his distinguished service. In character, he seems to have been a more prudent Colonel Newcome. Edward ("Omar") Fitzgerald and he were old friends. He died on February 26, 1848, aged 77.

seems to have had a good deal) to further investigations. He had already confessed himself baffled as to the cause of the bell-ringing in his own house, and to this he gave no further attention; but he made it his business to track down, as far as possible, the particulars of any other similar cases.

He seems, when he began his researches, to have been under the impression that the bell-ringing at Great Bealings was an entirely novel phenomenon. To him it probably was. Nowadays, those who are well-read in such matters would probably classify the Bealings bells as a somewhat unusual, but not unique, variety of the manifestations generally associated with the "poltergeist"—that irritating being whom sceptics regard as being, invariably, a mischievous child or servant; while those more credulous consider him an "elemental," possessed of a somewhat crude sense of humour.

During 1834-41 Moor accumulated details of some thirty cases which he regarded as similar to his own, and in the latter year he published his results in a small book entitled *Bealings Bells*.^{*} The motive for its publication was, avowedly, the very common one of trying to make a little money—but for a "worthy object." A new church was to be built at Woodbridge, about five miles east of Great Bealings, and it was hoped, with the usual optimism, that the present "scanty funds" would be augmented as the result of a bazaar. *Bealings Bells* was written for sale at that dismal function. I have no information as to the result, although this can perhaps be inferred from the fact that the book is extremely rare.

^{*} BEALINGS BELLS. // An Account / of the / Mysterious Ringing of Bells / at / Great Bealings, Suffolk, / in 1834; / and in other parts of England: / with Relations of farther / Unaccountable Occurrences, / in / Various Places: / by / Major Edward Moor, / F.R.S. &c. // Woodbridge: / Printed and sold by John Loder, / For the Benefit of the New Church. // 1841. (12mo. xiii + 142 pp.)

Such being the case, it seems worth while to compile from Moor's monograph—which, after all, is the *locus classicus* of a singular and rare phenomenon—an account of what happened at Great Bealings, and an outline of some of the other cases collected by him. A good deal of selection and compression is necessary. The book is not very well arranged, and details are alternately piled and withheld in a most exasperating manner. What conceivable purpose, for example, is served by beginning a chapter* like this?

“No. 29. THE ——— HOUSE GHOST

“The facts I am about to tell, belong to ——— House — or ———, as it was formerly called; a respectable old manor house in the north-eastern part of ———shire. It was, in very early times, the seat of the ———; a family of some distinction in the County.”

Some of the cases cited as analogous, too, are quite otherwise. Moor solemnly sets down particulars of two cases in which public clocks have struck more often than they should, and ascribes this to the same mysterious agency which pealed his own house-bells. We are also told of a quite ordinary “haunted house” at Windsor (strongly suspected to be a fraud); of a poltergeist at Sydersterne, Norfolk; of the apparition of a man on horseback, both “of colossal stature” and seeming “as if they had been flayed alive,” which appeared to two Army officers out shooting; and of mysterious thuds, like those of a pile-driver, which were at first thought to be produced by beetles. In the last-named case the reader's natural curiosity as to their species is gratified by a

* *Loc. cit.*, p. 112.

footnote: "Not the insect; it is believed—but the implement.—E.M."*

Even when dealing with the disturbances in his own house, Moor is sometimes a little difficult to follow. For example, he gives the most meticulously-detailed description of the arrangements of the bell-wires; yet it is, so far as I can judge, quite impossible to construct from this description a diagram showing that arrangement. It may be as well to point out that his bells, like those in every case which he describes, were of the old-fashioned kind, hung on spiral springs, and rung by pulling; this pull being transmitted from the "bell-pull" by means of thin wires led either through detachable casings secured to the walls of the house, or inside the walls themselves. The direction of the wires was changed where necessary by "bell-cranks"—L-shaped levers, pivoted at the angle of the L, and having the wires made fast to the two arms. Drawing out the bell-pull† about half an inch is usually enough to ring all such bells; on releasing the pull, a spring returns the wires to their original position.

But while it is easy to censure Moor's book as badly arranged, and its author as pedantic—and, in spite of the letters after his name, unscientific—there is another side to the picture. He wrote at a time when such subjects were not taken very seriously; he took a great deal of trouble to investigate various recondite matters which, but for his labours, would not have been permanently recorded; he may not always, or even frequently, get

* An engineer friend of mine once contributed to *The Times* an account of the demolition of a lighthouse, concluding with the statement that the last few stones had been removed by crabs. This excited so much astonishment that he was compelled to explain, in a later issue, that they were steam-crabs.

† Bells rung from outside a house generally have horizontal bell-pulls: inside the house the wires are usually pulled by a crank-handle.

hold of the right end of the stick, but it is his own painstaking record of his labours that enables this to be seen; he is perfectly frank and obviously honest. And he has a very remarkable story to tell.

Returning from church on the afternoon of Sunday, February 2, 1834, he was told that the dining-room bell had been rung three times between two and five o'clock. There was no one then in the room, and no apparent cause for the ringing. On Monday, much the same thing occurred. Moor considered that birds must have shaken the bell-wires—these, at one end of the house, ran outside it, and close to a pear-tree much frequented by blackbirds. On Tuesday the 4th, returning home about 5 p.m., he learned that five of the nine bells hung in the kitchen had been ringing peals, at intervals of about a quarter of an hour, for the last two hours.

With his son, he visited the kitchen, and while he was looking at the bells, they rang again, very violently—so much so that, although he knew what to expect, he was greatly startled. Ten minutes later, the same thing happened again, not quite so violently; and a third peal followed some fifteen minutes later. This went on, the interval between the peals gradually increasing, until a quarter to eight, when the disturbance stopped, except that about an hour after silence had fallen an attic bell, hanging by itself in the kitchen, sounded gently.

For a period of fifty-four* days—February 2 to March 27, 1834—these strange ringings continued. Their character was more or less uniform—peals of five bells, or of a different three, varied by occasional rings from the single bell mentioned previously, or from another single bell hanging in an attic, and connected

* Moor, in a footnote to p. 68 of his book, calls this fifty-three days.

with a bedroom. A curious feature of the peals was the extreme violence with which the bells were agitated—this could not be duplicated by ringing them from the bell-pulls in the ordinary way.

Moor does not state whether the bell-pulls and/or the wires (which could be sighted along practically their entire extent from pull to bell) were observed to move when the bells were pealed. But he is careful to point out that, so far as he could judge, no human agency could have produced what he heard.

“The bells rang scores of times when no one was in the passage, or back-house, or house, or grounds, unseen. I have waited in the kitchen, for a repetition of the ringings, with all the servants present—when no one—hardly ‘so much as a mouse’—could be in concealment. But what matters?—neither I, nor the servants—singly or together—nor any one—be he whom he may, could or can, I aver, work the wonderment, that I, and more than half a score others, saw. . . .”*

“I will here, note, once for all†—that after much consideration, I cannot reach any procedure by which they (these effects)‡ have been, or can be, produced.

If I had a year to devote to such considerations, and the promise of a thousand pounds in the event of discovery, I should despair of success. I would not, indeed, attempt it.”§

At the time of the ringings, he had stated, in a letter to the *Ipswich Journal*:

* Pp. 17, 18.

† The punctuation is that of the original: I am inclined to blame the printer, who seems to have followed the old tradition of “three stops to every line.”

‡ This is interpolated.

§ P. 21.

“February 25, 1834.

“P.S. I had on the above date (25.ii.34) an opportunity of reading the above proceedings . . . to six or eight very intelligent gentlemen at Woodbridge—and add, as my answer to some of their queries—that I keep no monkey—that my house is not infested by rats—that the wires of the five, and of the three, *pealers*, are visible in their whole course, from their pull to the bells, save where they go through walls, in which the holes seem no bigger than is necessary. The wires of the two single bells are also visible, except where they go through floors or walls. One or two of my friends, said it was ‘*all a trick.*’ It is *possible*. I have for many years of my life passed over large arcs of the earth’s surface, and have seen divers tricks of distant people. If this be one, it surpasses all that I have seen. . . .”

Needless to say, the theory mooted by his friends had several supporters—notably a correspondent* of the *Ipswich Journal* who appears, from his letter,† to have been a retired acrobat living in a house designed by the late J. N. Maskelyne.

“In my house, which compared with Mr. Moor’s, is of a limited extent, a person may, in a second, set three bells ringing violently by touching the wires, which all pass along a passage and through a hole in the wall, and in less than two seconds may get out of the passage at any one of the five doors, and may almost instantaneously set other bells ringing, and within two seconds may enter the kitchen in a different direction from that of the passage where the three bells were set ringing.”

* One still sees his name in the papers. He signs “Constant Reader.”

† *Loc. cit.*, p. 11.

He suggested to Moor, however, a very sensible plan of action—namely, to send for a few trustworthy neighbours, seat his entire establishment in one room with a friend, lock them in, station a friend on each staircase, and search the house, locking each room as he proceeded. “If this plan be pursued I will . . . make any moderate bet, either that the bells will not ring at all during the search, or that, if they do ring, the party searching the house will find some relative, or friend, of one of his establishment concealed in some part of the house.”

It would certainly have been more satisfactory if this had been done. But the letter did not commend itself to Moor. He remarks, rather loftily: “. . . I did not in any way, follow the advice therein offered.” It must be remembered, though, that he had previously satisfied himself that the bells could not be rung from any of the rooms in a way which reproduced the violence of the mysterious peals; and he also ascertained that they could not be so rung even by tampering with the wires at a point intermediate between the bell-pull and the bell.

To do this he tried the effect of pulling down with a hoe the wires of the five “pealer” bells at a point where they all ran parallel, and close together, along a passage between the kitchen and the back-house. By his description, it seems to have been about the most favourable spot at which to pull if one wished to disturb the bells—although no one could do so without being observed from the kitchen. The effect of his test was to show that if he pulled the wires violently the bells did not ring at all; while if he pulled them gently, the bells would tinkle, but no more. He admits, though, that it took less force to set the bells in motion than he would have expected.

He also admits that there is one point, that at which

the five wires first converge, from which they could be made to ring.

" . . . A rope led horizontally . . . and so pulled would ring them. But they could not be rung with the violent jerks witnessed and described. With no vigour of pull could that be effected. The cranks and wires might be broken: and I have no doubt but they would be, with very much less violence of pull. This is the only mode that occurs to me by which I could, even gently, ring the four or five bells. I confess, however, that the ringing in the passage, by the direct downward pull, moved the whole six bells into sound, easier than I had foreseen, or expected, before trial.*

"But it does not shake my expressed conviction—that my bells were not so rung by any mortal hand."

He is very non-committal as to any possible explanation; he gets no nearer to one than this extraordinary rigmarole:†

"The question ever recurs—what can be the cause? An adequate cause must exist; for these effects, and for every effect; moral and physical, in nature. But, in this case, no one has yet pretended, so far as I know, to develope (*sic*) it.

"It may be no advance to say—that, *possibly*, some hitherto undiscovered law of electricity or galvanism—latent—brought into activity, only by certain combinations of metallic alloys, in certain co-extension of parallelisms, straightness, or angles—certain concurring, or varied, degrees of tension—in connexion with certain conditions of atmospheric influences—acted upon by agencies, subtle and occult, &c., &c.

* He afterwards ascertained that no downward pull, applied anywhere along the whole run of the wires, could do more than tinkle the bells.

† *Loc. cit.*, pp. 57, 58.

"These possibilities—whose combined eventualities may, or may not, ever be developed—may be only another link in the amazing chain of results, that recent researches into the mysterious operations of science and galvanics, have brought under the wondering eye and contemplation of chymical philosophy.

"Who can say, or imagine, where they are to end? . . ."

There is a good deal more of this sort of thing, which may be all very well for the wrapper of a patent medicine, but seems a little out of place in a book written by a Fellow of the Royal Society.

I turn to the cases collected by Moor. He omits—whether designedly or otherwise there is nothing to show—the bell-ringing at Sampford Rectory in 1810, of which an account was published by "Lacon" Colton, who then held the living.* Some of his instances, too, are so shorn of detail by excisions that they are of no value; in one or two cases, no details seem to have been vouchsafed. The following, for example, might as well not have been printed:

"No. 19. A gentleman, whom I do not know, called on me, and stated that his father took a house in the country, that had been a lady's school.—The dinner-bell frequently rang during the night; and the bells in the house, were frequently ringing.—He tried an experiment, by fixing a bell to the wall; and it rang. . . ."

Two of the cases, however—the ringings at Greenwich Hospital in 1834 and at Stapleton, near Bristol, in 1836—deserve mention.

* *A Plain and Authentic Narrative of the Sampford Ghost* (1810). There is a copy of the Appendix to this in the British Museum, but none of the pamphlet itself. Colton's bells rang all night, apparently untouched.

The Ringings at Greenwich.

The best authority for the Greenwich ringings is a letter from Lieutenant William Rivers, R.N.,* in whose rooms they occurred, to Major Moor. His account is supported by a very similar one written by his wife, and by contemporary newspaper reports. I may say that I heard something of the story, as a still-lingering tradition, when I was at Greenwich Hospital (now Greenwich Naval College) in 1911.

(*Rivers to Moor.*†)

GREENWICH HOSPITAL,
April 26, 1841.

“The bells in my apartments in Greenwich Hospital, from some unknown cause, commenced ringing at half-past six o’clock, on the morning of the 30th September, 1834; and continued, first one, and then another, at intervals of four or five minutes, and sometimes all four at once. The first day, I had a minute examination made by the clerk of the works, and the bell-hanger; and in the evening, at eight o’clock, I had the wires cut off from them. The bells then ceased to ring; but the wires were agitated for some minutes afterwards. All remained quiet during the night. At nine o’clock next morning, the bell-hanger came, and re-united the wires to the bells; which had no sooner been done to the first, when it rang; the second the same; and they continued at intervals, as before, all that day: and many persons witnessed the performance.

* Rivers (b. 1788) had served in the *Victory* at Trafalgar, where he lost a leg. He was Warden of Woolwich Dockyard from 1824 to 1826, when he became a Lieutenant of Greenwich Hospital. He died there in 1856.

† The punctuation of this letter, and Mrs. Castle’s, departs so widely from accepted rules that I have had to alter it. The printer appears to have run short of commas, and to have used semicolons instead (*loc. cit.*, pp. 81–83).

“In the evening, about eight o’clock, I tied up the clappers; while so doing, the bells were much agitated and shook violently. They ceased to ring during that night. In the morning I loosed them again; when they began to ring again. The clerk of the works, his assistant, and Mr. Thame the bell-hanger came and had another examination, without discovery as to the cause. They requested the family and servants would leave the apartments to themselves. We did so, and dined at four o’clock at our neighbours opposite; and while at our dinner there we heard the bells ring a peal. Mr. Thame, and the assistant to the clerk of the works, remained until eleven at night, one watching the cranks, the other the bells below, with perfect astonishment: but they* ceased at their accustomed time, about nine o’clock or half-past. At eleven o’clock I requested them† to retire, having made up my mind to sleep there by myself; but my brother-in-law, Capt. Watts, and my wife, determined likewise to do so. I searched the apartment before I went to bed, and retired at half-past eleven o’clock. In the morning they began to ring again; but more faintly than before. I was then fully resolved to let them have their play out. . . . [They stopped about 3 p.m. on Friday, October 3rd.]

“I must here mention, that what appeared most extraordinary was the movement of the cranks, which (the bell-hanger said) could not cause the bells to ring without being pulled downwards; which they did, of themselves, in every room, working like pump-handles. . . .”

* The bells.

† Thame and the other man.

The Ringings at Stapleton.

The account which Moor prints of the Stapleton case* is contained in a letter from a Mrs. Castle (who, with her husband, was living in the house where the ringing occurred) to a Mrs. Shawe, of Kesgrave Hall, Woodbridge, by whom it was forwarded to Moor.

STAPLETON GROVE,
May 8, 1841.

“. . . One afternoon in July 1836, the bell of one of the sitting-rooms was observed to ring loudly several times; no person having touched it. In the course of half an hour the same thing occurred with nearly (if not) every bell in the house. Sometimes one would ring singly; then three or four together. The wires were distinctly seen to descend, as if pulled violently.

I sent for the bell-hanger; but before he arrived, the noise had ceased. He examined all the wires, without being able to discover any cause for this singular occurrence; and was about to take his leave, as it was growing dusk, when the bells again began to ring more violently than before. One we particularly noticed at this time, belonging to a room immediately over the passage in which the bells hang. It is pulled by drawing up a little slide against the wall; and the wire merely passes through the floor to the bell below. The slide we watched for more than five minutes. It was constantly shaken; even making a rattling noise, and the bell ringing.

When it had continued about an hour, I desired the bell-hanger to take down every bell, as our only chance of passing a quiet night. The maid-servants (who, as

* *Loc. cit.*, pp. 97-99.

you may imagine, were a good deal alarmed) assured me that the wires continued to shake through the night; but I cannot vouch for the correctness of this statement, and think it was probably a little fancy on their parts. The weather was rather hot, but we were not aware of any thunder during that week.

I think it impossible that there could have been any trick, as I assembled all the servants in one place, and had the house thoroughly searched. The bells had all been newly hung* about twelve months before with stout copper wire. They were all replaced the next morning, and have never shown a disposition to be riotous from that time. . . . We have always supposed it to have been caused by electricity.

“MARY CASTLE.”

In all such cases—and, indeed, in all disturbances of the poltergeist type—there must be a very strong presumption that the whole thing is the result of human agency. If a servant, or a child, has a grudge against the occupants of a house—or if any one desires to unsettle them, or to induce them to quit it—their bells offer a simple means (at least, for a time) of causing them a maximum of annoyance at a minimum risk to one's self. Nowadays, of course, the best method is to keep on ringing them up on the telephone—although if one takes the obvious precaution of using a series of call-boxes it is likely to prove a somewhat expensive amusement. But in the days before telephones, or even electric bells—and the Bealings, Greenwich, and Stapleton cases all occurred between 1834 and 1836—the facilities which house-bells offered to persons desirous

* I.e. the operating wires renewed. The bells are actually hung on spiral springs, formed from flat steel strips about an inch wide.

of paying off a score of any kind were certainly second to none.

If the disturbances at Bealings, Greenwich, and Stapleton had been of short duration, there could be little doubt that each was either a practical joke or a piece of spite. It is their duration which makes them remarkable. The longer they went on, the more attention they would attract; and, consequently, the greater would be the risk of the operator being discovered. Besides, familiarity, as we all know, breeds contempt; if he did not effect his purpose in a short time, he was not likely to do so by persisting with what had ceased to be a startling and unnerving thrill, and had become merely a nuisance, to be inquired into and, if possible, abated. It is easy to imagine many ways in which a fraud might have been perpetrated for a short time; but it is difficult to conceive why it should have been persisted in for so long—and still more so how the *modus operandi* failed to be discovered once curiosity was thoroughly aroused. Moor, as we have seen, was most thoroughly convinced that no human agency moved his bells; the minions of the Board of Works were equally baffled at Greenwich; Mrs. Castle took effective measures which certainly should have exposed a mere trick.

As the Senior Wrangler remarked of the *Ode to a Nightingale*, such occurrences, however well authenticated, "prove nothing" and lead nowhere. They may have some natural explanation—most probably there is one, if we knew exactly where to look for it. On the other hand, it is an indisputable fact that the persons chiefly desirous of finding such an explanation, who may be presumed to have known more of the facts than we can hope to do now, were not successful. If Major Moor and the others were merely the victims of a pro-

longed and annoying hoax, its perpetrator may at least be congratulated upon having outshone Cagliostro, the Davenport Brothers, and many "mediums" of our own day—in that he went to his grave undetected and unexposed.

THE STRAIT OF ANIAN

AN earnest but simple-minded lady is credited with the remark that 'she thought she could understand how astronomers discovered new stars, and so on, but that it was a mystery to her how they found out their names. Although it would have been rather like "robbing a blind kitten of a dead mouse," it is a pity that no one told her of an even more wonderful fact; namely, that the navigators and geographers of the sixteenth century had found no difficulty in giving a name (which attained considerable celebrity) to a strait which not only was not then discovered but which, as a matter of fact, has no actual existence.

When Gaspar de Corte Real, in the year 1500, crossed from the Azores to Newfoundland and thence made his way into the Gulf of St. Lawrence, he started, all unconsciously, a geographical myth which was to hold sway over men's minds for nearly three centuries. In good faith, he took the St. Lawrence (which he explored for some distance westward) to be a strait communicating with the Pacific; and he named it, or is believed to have named it, the "Strait of Anian," after two brothers who sailed with him.*

Thereafter, the Strait of Anian became an article of faith with geographers—in the sense that they firmly believed in its existence although (as in all matters of faith) definite proof was lacking. Corte Real was wrecked

* Others derive the name from Marco Polo's "Gulf of Cheinan."

and lost on his next voyage, but the name which he had given persisted; it even survived the examination of the St. Lawrence made by Cartier in 1535 and subsequent years, during which he ascended that river as far as what is now called Montreal, and proved that Corte Real's supposed strait had no real existence. Magellan's circumnavigation of the world in 1519-22* had demonstrated two things: that there was an ocean, as surmised by Balboa, to the westward of the New World—and that the way to it, and to the Oriental markets, round the south of that continent involved a very long and dangerous voyage. His doings, therefore, focused attention on the advantage to be gained if America could be rounded "north-about"—if a channel could be discovered, north of the Equator, connecting the Atlantic with the Pacific.

In those days geographers were not so much hampered by facts as they are now. The map of the world, although its broad outlines were known to some extent, was still very much like a jig-saw puzzle, of which the pieces could be put together in a number of ways and still fit. Theory, to a great extent, ruled the roast, and if facts conflicted with it, so much the worse for them. It seemed probable that a northern passage from the Atlantic to the Pacific ought to exist—there was no known reason why it should not exist—ergo, it did exist. The name "Strait of Anian" had already been given to a strait of the precise kind required—it was as good a name as any other—it should be retained. Only, since it appeared that what Corte Real had taken for such a strait was not the real thing, it seemed, on the whole, more likely that the latter should be charted as running

* It seems too pedantic to speak of del Cano's circumnavigation of the world, although it was he who brought the *Vittoria* home, Magellan having been killed at Matan.

somewhere between the known north-eastern coast of Asia and the unknown north-western coast of America.

Whether the early sixteenth-century geographers reasoned in this manner is doubtful. But they certainly acted as if they had done so. On Ribero's map of 1529 the "Strait of Anian" does not appear; but it is shown on Zaltieri's map of 1566, on Mercator's of 1569, on Frobisher's map of 1578, and on various others.* All these unite in locating it in approximately the position of Bering Strait (not "discovered" until 1728, although it must have been traversed by Deshnef in 1648). Some of the maps, in addition to showing and naming the strait, also indicate a "kingdom of Anian" on one or other shore of it. It has been suggested that these indicate that the name is possibly of Chinese origin, and support appeared to be given to this view by the discovery, in the Ambrosian Library, Milan, of a Chinese planisphere of the late sixteenth century, which showed the strait (in the Bering Strait region) and close to it, in Chinese characters, the names "Yanian" and "Kingdom of Yanian." Unfortunately, it seems only too probable that the map in question is the work of Father Matthew Ricci, who spent some twenty years instructing the Chinese in Western arts and sciences, and died at Peking in 1610.†

But if geographers were agreed that the Strait of Anian lay between Asia and America, navigators were by no means disposed to relinquish Corte Real's theory that it traversed North America, and many of them did their best to translate their opinion into fact. Drake's voyage

* The map of A.D. 1600 which Shakespeare calls " . . . the new map, with the augmentation of the Indies," bears a note referring to " . . . the sea between the west part of America and the east of Asia (which hath bene ordinarily set out as a straight, and named in most maps the streight of Anian . . ."

† Attention was first drawn to this map by Amoretti in 1812, in his work on Maldonado, referred to later. He admits that the map is probably drawn by Ricci, but prefers to believe that its information came from Chinese sources!

round the world, the third on record and the first performed by an Englishman, provides a striking example of this. When Drake had captured the treasure-ship *Cacafuego*,* and made the Pacific coast of Central America too hot to hold him, he formed the bold plan of running far to the northward along the American coast, and making his way, if possible, eastward from the Pacific to the Atlantic, thus avoiding the return journey through Magellan Strait, where the Spaniards were not improbably waiting for him in much superior force. At that time the eastern side of the Pacific, northward of Mexico, was almost unknown, and it says much for Drake's courage and resolution that, with a crank ship, and a load of treasure to breed discontent and mutiny among his men, he should have made his way to lat. 48° N., and only turned back when the increasing cold, the north-westerly trend of the coast, and the persistently adverse wind made it clear that his boldest but safest plan was to up helm and set his course back to England west-about round the world.

But if he had not found the strait he was seeking, he had left unsearched an enormous area in which it might lie; and during the era of discovery which his voyage had inaugurated for English seamen many efforts were made to find the famous "North-West Passage" which should shorten the way to the "South Sea"† and so to the Indies. Simultaneously, efforts were also made in two other directions: to find a route along the north coast of Asia—the "North-East Passage"—and to sail across the North Pole itself.

With the north-eastern and Polar voyages I am not directly concerned, although it is worth noting that in

* So his men called her. She was really the *Nuestra Señora de la Concepcion*.

† The Pacific.

1580 Pet and Jackman fought their way as far as the Kara Sea, along a track which many modern voyagers have failed to follow,* in two tiny vessels of 40 and 20 tons respectively: and that Hudson, in his voyage of 1607, reached a latitude of approximately 81° N., and was hence within six hundred miles of the Pole. His "furthest north" was not surpassed until 1773.†

During the period 1576-1632 some twenty voyages were made by English seamen (the most notable of whom were Frobisher, Davis, Hudson, and Baffin) for the discovery of a north-west passage. All failed; but their failure was more creditable than many successes. In our own day it is hard to realize the dangers and difficulties which our Elizabethan forefathers regarded as part of their trade. In tiny ships, undermanned by mutinous crews, ill-found and ill-equipped by private enterprise, with small hope of reward and something more than an expectation of total loss, they waged incessant warfare with all the powers of Arctic nature for a full half-century, and only rested from their labours when it became evident that even if the passage they sought did exist, it could never be of any commercial value. One of the greatest of them all, Henry Hudson, summed up their ideal in a single noble phrase when he gave it as his opinion that men should "achieve what they have undertaken, or else give reason wherefore it will not be": and it was this stern conception of duty which led directly to his tragic and shameful death—turned adrift in an open boat by his mutinous crew.‡

* The North-East Passage was first accomplished by Nordenskjöld in 1878-79; the North-West by Amundsen in 1903-05.

† By Captain C. J. Phipps, R.N., with the *Carcass* and *Racehorse*. Nelson, then fifteen, served in the *Carcass*, and nearly lost his life in an encounter with a bear.

‡ It is pleasant to record that one of the loyal, forgotten men who died with Hudson showed himself a true hero. The mutineers were anxious to secure the

The early north-western voyages were not by any means barren of result—in fact, by disclosing the existence of the huge inland sea now called Hudson Bay, whose extent and completely land-locked character were not fully determined until more than a century later, they seemed to indicate that the northward extent of the American mainland was by no means so great as had been supposed, and that a channel between the Atlantic and Pacific might possibly exist in comparatively low latitudes. Definite proof, in fact, that such could not be the case was not given until 1771, when Samuel Hearne, in the employment of the Hudson Bay Company, made an overland journey northward through the unknown Canadian hinterland, reaching the shores of the Arctic Ocean at the mouth of the Coppermine River, in lat. 68° N.* Eighteen years later Alexander Mackenzie, afterwards famous as the first man to cross the Rocky Mountains, duplicated this feat by reaching, also overland, the mouth of the Mackenzie, in lat. 69° N.; while the gallant but obtuse Sir John Ross, in 1829, believed himself to have proved that the North American continent extended northward, in the form of the Boothia peninsula, to 74° N.—having, *more suo*, sailed past Bellot Strait (72° N.) without noticing it. Meanwhile, however, Vancouver (by his great voyage of 1790–95) had demonstrated, in conjunction with the work of previous explorers† of the north-western coast of America, that

services of Philip Staffe, of Ipswich, the ship's carpenter, and put it to him that he could save his life by throwing in his lot with them. His answer was, that he would rather die with true men than live with cowards and murderers.

* Owing to defective instruments, Hearne made this 72° N.

† Such as Portola (1769–70), Perez (1774), Heceta and Quadra (1775), Cook (1778), Arteaga (1779) and Malaspina (1791). It is noteworthy that Malaspina's instructions specifically directed him to search for a strait believed, even then, to run from the Pacific coast to Hudson Bay (whose western shore had been shown by Middleton, in 1742, to have no such channel running into it).

this stretched in an indented but unbroken line from California to Bering Strait—in other words, that even if a ship could find a passage around the shores of North America she must, in any event, traverse Bering Strait before she could enter the Pacific.

But from 1632 to 1790, or even to 1771,* is a far cry, and in the century and a half during which those who wished were still at liberty to believe in the existence of a "Strait of Anian" running from the north-west coast of America to Hudson Bay, or to the Gulf of Newfoundland, or wherever else they thought most suitable, accounts, varying in credibility but invariably vague as to geographical details, were not wanting of voyagers who had actually made this passage. Such "travellers' tales," as we know them to have been, had been current since at least 1560; and, in the absence of conclusive disproof, had found a certain amount of credence among geographers of that curious type of mind which will believe anything (on the flimsiest of evidence, or none) which fits in with a preconceived theory. Such a man was Joseph Moxon, who gravely placed on record, in a work published in 1674,† the *bierhalle* chatter of a Dutch whaling captain, who claimed (probably *blotto voce*)‡ that he had sailed so far north that he not only reached the Pole, but went a full degree past it.

Here is a short list, in chronological order, of these

* The journeys of Hearne and Mackenzie were not accepted by ardent believers in a practicable north-west passage (e.g. Meares and Goldson) as conclusive. Some part of their routes lay over frozen lakes and rivers—and it was suggested that these might, in reality, be arms of the sea.

† *A Brief Discourse of a Passage by the North-Pole to Japan, China, &c.* London, 1674.

‡ Moxon relates that he met this marine Munchausen at Amsterdam, in a "Drinking House," which he had entered "... to drink a cup of Beer for my Thirst."

apocryphal voyages through the Strait of Anian.* In the main, their interest is psychological rather than geographical; but there are two outstanding exceptions. The story of Juan de Fuca was based upon a real achievement which, most justly, has perpetuated his name on the charts: and the alleged voyage of Maldonado in 1588 offers an extremely interesting problem, of which it scarcely seems possible to formulate a definite solution.

Martin Chack.

Purchas, on the authority of one "Thomas Cowles of Bedmester† in Somersetshire," records that one Martin Chack (Chaco?) had found a way from "the Portugal Indies through the Gulf of Newfoundland," which he believed to lie in 59° N.

J. F. de Ladrillero.

In 1574 a Spaniard named Juan Fernandez de Ladrillero made an official declaration to the Spanish authorities in Mexico that he had discovered, in about 60° N., a strait which ran from the Pacific to the Atlantic, its eastern entrance being frequented by English fishing craft.

L. F. Maldonado.

In 1588, by his own account, Lorenzo Ferrer Maldonado claimed to have made a voyage through Hudson Strait, to lat. 75° N., and thence westward to Bering Strait and so into the Pacific; returning by the same route. No particulars of the voyage were made

* The list is chiefly compiled from Burney's *Voyages* . . . London (var. d.). Some particulars have been added from Meares's *Voyages to the N.W. Coast of America* (1790) and from various of the works of Alexander Dalrymple.

† Bedminster.

generally known at the time, and in later years it was entirely discredited. A MS. account of it, discovered in 1812, is discussed later.

Juan de Fuca.

In 1592, Juan de Fuca, a Greek, sailed from Mexico to discover and explore the Strait of Anian. The sole authority for this long-questioned voyage is an account given by Purchas* on the authority of Michael Lok (sometime Consul at Aleppo), who met de Fuca at Venice in April 1596. The following is an extract from de Fuca's narrative, abridged from Burney's abridgment of Purchas' abridgment of Lok's account of it.

" . . . the said Viceroy of Mexico sent him out again in 1592, with a small Caravel, and a Pinnace, armed with mariners only, to follow the said voyage for the discovery of the *Straits of Anian*, and the passage thereof, into the Sea, which they call the *North Sea*, which is our North West Sea.†

. . . he followed his course in that voyage, West and NW in the *South Sea*, all alongst the coast of *Nova Spania*,‡ and *California*, and the *Indies* now called *North America* (all which voyage he signified to me in a great map, and a sea card§ of mine own, which I laid before him), until he came to the latitude of 47 degrees, and that there finding that the land trended North and North East, with a broad inlet of sea, between 47 and 48 degrees of latitude, he entered thereinto, sailing therein more than twenty days, and found that land trending still sometime NW and

* *Purchas, His Pilgrimes*, Vol. I, Book III, chap. iii, par. 5.

† *I.e.* the North Atlantic—not the erstwhile "German Ocean."

‡ Mexico.

§ A chart.

NE, and North, and also East and South Eastward, and very much broader sea than was at the said entrance, and that he passed by divers islands in that sailing. . . .

And also he said, that he being entered thus far into the said Strait, and being come into the North Sea already, and finding the sea wide enough every where, and to be about 30 or 40 leagues wide in the mouth of the Straits, where he entered; he thought he had now well discharged his office: and that not being armed to resist the force of the savage people that might happen, he therefore set sail, and returned homewards again towards *Nova Spania*, where he arrived at *Acapulco*, Anno 1592, hoping to be rewarded by the Viceroy for this service done in the said voyage."

It is scarcely necessary to say that he was disappointed. No nation has precisely distinguished itself by liberality towards its explorers; but it is safe to say that none has ever equalled, or even approached, Spain in consistently treating them with ingratitude and contempt.

". . . after coming to *Mexico*, he was greatly welcomed by the Viceroy, and had promises of great reward; but that having sued there two years and obtained nothing to his content, the Viceroy told him that he should be rewarded in Spain of the King himself very greatly, and willed him therefore to go to Spain, which voyage he did perform."

It is rather singular that if he really believed himself to have found the Strait of Anian he should not have made it his route to Spain, instead of Magellan Strait. Can he have tried to do so and suppressed any mention of his failure?

“Also he said, that when he was come into Spain, he was welcomed there at the King’s court; but after long suite there also, he could not get any reward . . . therefore at length he stole away out of Spain, and came into Italy, to go home again and live among his own kindred and countrymen, he being very old.

Also he said, that he thought the cause of his ill reward had of the Spaniards, to be for that they did understand very well that the English nation had now given over all their voyages for the discovery of the North-West Passage, wherefore they need not fear them any more to come that way into the *South Sea*, and therefore they needed not his service therein any more.”*

He begged to Lok to write to England, offering his services to Queen Elizabeth, undertaking to “serve her Majesty in that voyage for the discovery perfectly of the North-West Passage into the *South Sea*, if she would furnish him with only one ship of forty tons burthen and a pinnace, and that he would perform it in thirty days’ time, from one end to the other of the Strait.” This somewhat ambitious programme (on his own showing de Fuca, although he proposed to reverse the direction of his previous voyage in the Strait, had no knowledge of where its eastern end—his new starting-point—was) was duly transmitted by Lok to Burleigh, Raleigh, and Hakluyt; de Fuca, meanwhile, leaving Venice on his way to his home in Cephalonia.

Not surprisingly, Lok was informed that “the proposed voyage “. . . was well liked, and greatly desired in

* The voyages to which he refers at that date (1596) must have been Fro-bisher’s three (1576, 1577, and 1578) and Davis’ (1585, 1586, and 1587). After Davis’ last voyage there was a lull in English attempts at the North-West Passage until Weymouth sailed for this purpose in 1602.

England, but the money was not ready"; an expression which I take to be the Elizabethan equivalent of the later formula: "My Lords Commissioners of the Treasury regret that they cannot sanction any such expenditure." He appears to have had some further correspondence with de Fuca in 1597 and 1598 about the proposed voyage, but this came to nothing; and when, in June 1602, "having recovered a little money," Lok wrote to de Fuca asking him to come to Zante and thence, as his guest, to England, he learned that the Greek was then dead or dying.

There is no reasonable doubt that de Fuca did make a voyage along the Californian coast, and northward, in 1592: that he entered the strait, in lat. $48\frac{1}{2}^{\circ}$ N., which now bears his name: and that he spent some time in navigating the intricate system of sounds and channels to which it gives access. It is quite natural that on a cursory examination he should have concluded that he had discovered the long-sought strait, and have hurried back to Mexico to be first with the news. What is more singular, is that his report should not have been followed up, and that it should be left for the navigators of the late eighteenth century to lay the ghost which he raised.*

Lancaster.

A letter from Captain James Lancaster to his employers, the East India Company, relating to the voyage of discovery (via the Cape) which he performed in 1600-1,

* His exploit also brought him the doubtful honour of having his name mentioned in almost every textbook relating to that marvellous collection of fairy-tales which is termed "International Law." The boundary between British Columbia and the U.S.A. runs through the centre of Juan de Fuca Strait, having been thus laid down by no less a person than the ex-Kaiser. Extending a long way beyond the 3-mile limit, it provides a fertile theme for the pundits whose business it is to wrangle over the nature and extent of "territorial waters"—*quorum parva pars fui*.

contains a postscript, of doubtful origin, which casually states: "The Passage to the East Indies lieth in $62\frac{1}{2}^{\circ}$ by the North West on the American side."

De Fonte.

A voyage somewhat resembling de Fuca's in its nature, and also in the doubt attaching to it, is claimed to have been made in 1640 by Bartholomew de Fonte, a Spanish admiral. The authenticity of the narrative, let alone the accuracy of its statements, is exceedingly doubtful; and, in view of the circumstances surrounding its publication, this is not in the least surprising.

It appeared in an anonymous periodical, published in London, called *The Monthly Miscellany, or Memoirs for the Curious*, part being published in April 1708, and the remainder in the following June. Although it appeared sixty-eight years after the (alleged) event, and was written in such a chaotic style that no one could easily form any clear idea of the events of the voyage, it was accepted as genuine by many contemporary and subsequent authorities. For instance, J. N. Delisle, of the French Academy, presented to that body a translation of the voyage, together with a chart (drawn by Philippe Buache and himself) showing de Fonte's discoveries. This, styled by Burney "as adventurous a piece of geography as was ever published," depicts a (hypothetical) channel running from the Pacific into Hudson Bay.*

Although hardly anything is known of de Fonte, and no trace of any such letter as that printed in the *Monthly Miscellany* over his name has ever been found in the Spanish or Mexican archives, there is some evidence that he was a real person who was employed in the Spanish service and carried out various explorations in

* See Fig. 11.



FIG. 11.—Delisle's chart of de Fonte's discoveries. Reproduced from Thomas Jeffery's *Voyages from Asia to America* (London, 1761).

the New World. Witson, in his *Nord en Oost Tartarye*, speaks of him as a celebrated Portuguese seaman, named "de Fonta," and testifies to having examined the original manuscript of a survey of Tierra del Fuego made by him in 1649.

On the other hand, it is not at all unlikely that his alleged voyage of 1640 had its origin in the fertile brain of one James Petiver, a naturalist who was a constant contributor to the *Monthly Miscellany*. Some notes of his are extant* which show that he was "well read in books of voyages"; and he is known to have written a *Voyage to the Levant* (published as fact in the same journal) without first going through the formality of making one.

If we take the story at its face value, de Fonte sailed from Lima with four ships on April 3, 1640, and made his way northward along the eastern coast of the Pacific as far as a river, which he called the "Rio de los Reyes," in lat. 53° N. Here he detached one ship, under Captain Pedro de Barnarda, to explore independently; while he himself made his way gradually eastward through a chain of large lakes connected by navigable straits, until, on July 17, 1640, he fell in with a "great ship" (commanded by one Captain Shapley) from Boston, New England! According to Shapley, his owner was "a fine gentleman, and Major-General of the largest Colony in New England (Massachusetts)." His name, like that of the vessel, is not stated.† De Fonte then turned westward, and was afterwards joined by Barnarda, who had succeeded in sailing north-eastward to a high latitude,‡ while one

* At the British Museum.

† Possibly on the Army principle of "no names, no pack drill"; especially since de Fonte had told him "... my commission was to make prize of any people seeking a north-west or west passage into the South Sea."

‡ Stated to be 77°! But the whole narrative is so full of inconsistencies—it reads more like a very imperfect translation than a hoax—that it is scarcely fair to single out this in particular.

of his men had been conducted by natives to the shore of Davis Strait.

It is possible that de Fonte may have reached 53° N., as he stated. In that case, he would find himself in what is now called Hecate Strait, B.C., and could have taken his choice of many inlets to explore. The encounter with the American ship, however, and Barnarda's Arctic explorations, can only be regarded as "corroborative detail, intended to give artistic verisimilitude to a bald and unconvincing narrative."

Peché.

Thomas Peché, an Englishman, is stated to have attempted in 1676 to return to England, after two years' trading in the Pacific, through the Strait of Anian. Unfortunately, we are not told its position: but apparently he had no difficulty in finding it, and in making his way along it for about 350 miles eastward. He then met with persistent northerly gales and strong currents, which induced him to turn back and make for Magellan Strait—where, in all probability, he encountered worse weather still. It seems possible that Peché's "Strait of Anian" was Bering Strait. Seixas y Lovera, the only authority for Peché's voyage,* quotes some incidental remarks by the latter which suggest that he had at least been as far north as the Aleutian Islands.

A rumour was current† at the Cape of Good Hope, about the time of Cook's third voyage (1776–78), that a strait had recently been discovered, with its western opening in the Pacific, in lat. $47^{\circ} 45'$ N., and its eastern

* *Theatro Naval Hydrographico*, chap. xi. According to him a narrative of Peché's voyage, in French and English, and forming a pamphlet of some twenty quarto pages, was published in 1679.

† It is mentioned by Dalrymple, who had it, *via* the Rt. Hon. George Greville, from Sir John Macpherson, who had heard it at the Cape. What could be more satisfactory?

in Hudson Bay. This seems to have been a canard based on de Fuca's narrative.

Gray.

Finally, it was asserted by John Meares in 1790* that an American sloop, appropriately named the *Washington*, had sailed through the Strait of Juan de Fuca into an extensive sea, occupying (so he suggested) a great part of the interior of North America. However, Vancouver in 1792 fell in near Juan de Fuca Strait with an American vessel which proved to be the *Columbia*, whose captain, Robert Gray, had been in command of the *Washington* at the time when she was supposed to have performed the wonderful exploit attributed to her. It turned out that Gray, as a matter of fact, had not advanced, or attempted to advance, further eastward than the inner end of the strait itself—and that the “inland sea” which he was supposed to have reported was a pure myth, for which he was in no way responsible.

It seems probable that Meares may have been honestly misled by hearing a garbled version of Gray's doings in the *Washington*, and that, being an enthusiastic champion of the theory that a commercial north-west passage existed, he simply snatched at what he took to be a useful piece of evidence in support of his views. But he certainly laid himself open to strong criticism, which he duly received. George Dixon, who had been on the north-west American coast at the same time as Meares (1786–87), fell foul of his book, and attacked it in two open letters, to which Meares replied.† The con-

* In his *Voyages . . . to the North-West Coast of America*, London, 1790.

† *Remarks on the Voyages of John Meares, Esq.*, by George Dixon (London, 1790); *An Answer to Mr. George Dixon*, by John Meares, Esq. (London, 1790); *Further Remarks on the Voyages of John Meares, Esq.*, by George Dixon (London, 1791).

the straits of John de Fuca, the knowledge of which she had received from us.' You also observe, that 'the track of this vessel is of great moment.' Be so good, Mr. Meares, as to inform the public from what authority you introduce this track in your chart?

"I have read your amusing and *instructive* book again and again, and cannot find any account of it; I therefore strongly suspect it never was taken from any actual survey, but has been introduced into your chart merely as a pretty variety to fill up a blank; and I cannot think of any thing it resembles so much as the mould of a good old housewife's butter pat."*

Goldson.

The Strait of Anian found its last champion in William Goldson, of Portsmouth. In a work published in 1793† he stood up manfully for the essential accuracy of both Maldonado's‡ and de Fonte's narratives; and in an accompanying chart,§ even more "adventurous" than that of Buache and Delisle, he managed to combine the actual discoveries of Hearne, Mackenzie, and Cook with various hypothetical channels across north-west America in a manner which at first sight seems to be "all ship-shape and Bristol fashion." Had Goldson stayed his hand for three years, his book, I imagine, would never have seen the light—for by 1796 it had become known that Vancouver had filled up the gaps in Cook's survey,

* See Fig. 12.

† Observations on the Passage between the Atlantic and Pacific Oceans, in two Memoirs on the Straits of Anian, and the Discoveries of De Fonte. Elucidated by a New and Original Map. To which is prefixed an Historical Abridgment of Discoveries in the North of America. By William Goldson (Portsmouth, 1793).

‡ He knew of Maldonado's voyage from a paper read before the Académie des Sciences (13.IX.1790) by Philippe Buache, who had seen a copy of the Cadiz version of Maldonado's *Relation*.

§ See Fig. 13.

and that the American coastline was continuous from California to Bering Strait. Goldson's, I believe, is the last printed map which shows the Strait of Anian.

Such, in outline, is the history of that famous Strait. Located first of all in the Gulf of St. Lawrence; shifted thence to the locality of the then-unknown Bering Strait;*

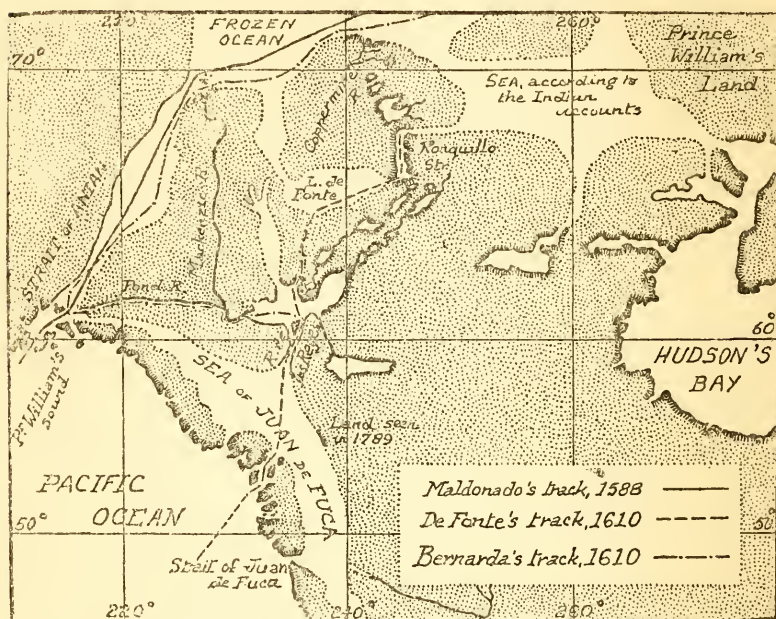


FIG. 13.—Sketch showing the limits and outlines of Goldson's map. The original measures about 20 in. by 16 in.

The shaded outlines correspond quite well with geographical fact, as known in 1793: the dotted outlines represent Goldson's conjectures (based upon Maldonado and de Fonte).

and finally given a "local habitation" somewhere along the north-west coast of America, the hare which Corte Real had put up in A.D. 1500 was not run down and worried until the beginning of last century. He might at

* It is of little use trying to eradicate persistent errors; it generally gives them fresh vitality. Still, I should like to point out once more that there is no such place as "Behring" Strait. Its discoverer wrote his name Veit Bering.

least claim that his mistaken notion provided a powerful incentive which contributed greatly to the advancement of geography.

The Voyage of Maldonado.

I turn to the exceedingly curious circumstances attending the alleged voyage of Lorenzo Ferrer Maldonado to the Strait of Anian (or, rather, to Bering Strait) in 1588, the year of the Armada.

In his own day, and much later, Maldonado seems to have been generally discredited as an impostor—or at best a dreamer. His very existence has been doubted—but unreasonably. Nicolao Antonio, in his *Bibliotheca Hispana*,* speaks of him as follows (I translate):

“Lorenzo Ferrer Maldonado, having undertaken a military† career, occupied himself with the studies proper to an officer, and deserved well both of the nautical art and of geography. He wrote a book entitled *The Image of the World upon the Sphere: a Cosmography and Geography. together with the Art of Navigation*. Published by John Garcia, 1626, in quarto. (Also) *A Relation of the Discovery of the Strait of Anian by the Author*. This Relation I have seen in MS. at the house of Don Jerome Mascaregnas, Chevalier of the Royal Military Order, sometime Senator of the Council of Portugal, and at present Bishop of Segovia.

“The author states that he made this expedition in 1588. According to the statement of Antonio de Leon, in his *Bibliotheca Indica*, he was to be counted among those who gave those Senators of ours who

* Part II, vol. ii, p. 3. (Written 1672—published at Madrid, 1788).

† In the original amphibious sense; cf. the present Military Branch of the Admiralty.

direct the affairs of the Indies ground for hope that they could discover a compass, or compass-needle, which was not subject to the ordinary variation; and, by means of certain measures and observations, a method of determining the degrees of longitude in navigation; but the result was not commensurate either with their trouble or with their expenses."

This statement as to Maldonado's researches is corroborated by two entries* concerning them in the Spanish treasury accounts of the period, which record certain payments made to him:

"(8. VIII. 1615.) To Captain Lorenzo Ferrer Maldonado an allowance of 40 escudos a month while afloat for observations on the longitude and the invariable compass, for which he was promised a perpetual pension of 5,000 ducats.

"(—, 1626.) Lorenzo Ferrer Maldonado was given 200 ducats for making certain observing instruments."

But whatever reputation Maldonado may have gained as a writer on navigation, it is certain that in his own time he was of no account as an explorer. His *Relation of the Discovery of the Strait of Anian* remained unpublished for over two centuries: until 1812 few geographers had so much as heard of his supposed voyage, and those who referred to it (e.g. Humboldt) dismissed it, on hearsay evidence, as unworthy of notice. Almadover† remarks:

"This voyage has never been printed, and the manuscript of it has remained buried in the dust of

* Quoted from Duro's *Disquisiciones Nauticas*.

† He appears only to have seen an extract from the MS. He was inclined to accept the voyage as genuine, but was doubtful as to the accuracy of its details. A copy of the MS. seems to have turned up at Cadiz about 1790.

the archives, suffering the similar fate of many others, either through negligence, or through the principles adopted by our Government. . . .”

It is quite possible that Maldonado's original MS. is still at Madrid, but it has never seen the light. In 1812, however, Charles Amoretti, Librarian of the Ambrosian Library at Milan, discovered on his shelves an MS. copy of Maldonado's *Relation*. He deduced, from the watermark, that it had probably been made at Milan; and, from the writing, that it dated from the late sixteenth or early seventeenth century. It was written in Spanish, but contained many clerical errors. Convinced of its authenticity, he immediately published it,* accompanied by an erudite but not very judicial commentary in which he endeavoured to support the literal accuracy of the whole narrative—a task which, as Mark Twain puts it, “would oversize anyone's knowledge.”

Assuredly, if we accept Maldonado's statements respecting his voyage, it was one of the most remarkable ever made. By his own account, he made his way up Hudson Strait, up Fox Channel into Prince Regent Inlet, thence to Lancaster Sound, and westward along the coast of America to Bering Strait, through which he passed into the Pacific. Not only did he do this, but after spending some time exploring the Bering Sea, he returned as he had come (without, apparently, any obstruction from ice) in the same season. In other words, he claims to have made the North-West Passage twice in one year—an all but incredible statement in the light of modern knowledge; for Amundsen took nearly three years to

* “Voyage / de la Mer Atlantique a l'Ocean Pacifique / par le Nord-Ouest dans la Mer Glaciale / par le Capitaine / Laurent Ferrer Maldonado / L'An MDLXXXVIII / Traduit d'un manuscrit espagnol / et suivi d'un discours / qui en démontre l'authenticité et la veracité / par Charles Amoretti.” Plaisance (Piacenza) de l'imprimerie del Majno. MDCCCXII.

accomplish it from east to west, while no ship has ever made it from west to east.*

On the other hand, it must be admitted that if Maldonado's story is a mere romance, it agrees with some of the facts which we now know (and which, on this hypothesis, he could not have known) in a very remarkable manner. I do not propose to follow Amoretti in his elaborate defence of Maldonado, but it may be as well to quote here the most important portions of the latter's narrative. It begins as follows (I translate):

"A RELATION of the discovery of the Strait of Anian, made by me Captain Lorenzo Ferrer Maldonado in the year 1588,† in which one reads the navigation of that place, the characteristics of it, and the manner of fortifying it.

"There are also discussed the advantages of this navigation, and the losses which will result if it be not seized."

He begins by pointing out the great advantages which will ensue by making use of his "Strait of Anian" (which, as already stated, corresponds with Bering Strait), and urges that it should at once be occupied and fortified by His Most Catholic Majesty in order to forestall similar action on the part of any other Power, which would then be in a position to do incalculable harm to the Spanish possessions in the New World.

He proceeds to give a short account of his route, and of the strait.

* McClure, who passed Bering Strait in 1850 in the *Investigator*, reached Mercy Bay, Banks Island, in the following year, but was compelled to abandon his ship there in 1853. His crew, suffering severely from scurvy, completed the N.W. Passage by crossing on foot to Dealy Island, whence they returned to England with Belcher's expedition.

† The MS. must have been written many years later, for it refers to Quiros' discoveries, which were not known until 1607.

"You sail from Spain; let us suppose, from Lisbon. You steer north-west for a distance of 450 leagues. You will thus arrive in 60° N. latitude, where you will sight the land formerly called *Thule*. . . . When you have sighted Friesland, you set a course to the westward, keeping in lat. 60° N., and running thus 180 leagues to the land of Labrador, where the strait of Labrador, 30 leagues wide at the entrance, begins. . . . You must then steer North-west for 80 leagues, until you reach the latitude of 64° N. or a little less.

"Here the strait changes its direction, and it is necessary to alter course to North, and to run thus for 120 leagues, until one is in 72° N. latitude.* then the direction of the strait alters again, and the channel runs north-west; you run along this channel for 90 leagues, and find yourself in about 75° . Here one emerges from the strait of Labrador; which, as I have said, begins at 60° and ends at 75° , its length being 240 leagues, and forming three elbows or arms of the sea, of which the first and the last run south-east and north-west, and the middle one south and north. . . . As far as 73° , it is inhabited, for we several times saw men on its shores. . . .

"After clearing the entrance of the strait of Labrador, you begin to lower your latitude, and steer W.b.S. for 350 leagues to lat. 71° . It was in this neighbourhood, during our voyage, that we discovered a very high land, but we could not well determine whether it were the mainland or an island. However, we concluded that if it were mainland, it was joined to New Spain.†

* It is often difficult, in accounts of old voyages, to determine what species of "league" is used; but from this it is obvious that Maldonado's is the "German league" of 4 nautical miles. Burney suggests, on this ground, that Maldonado's *Relation* was really written by some Fleming.

† The American continent.

“Having sighted this land, in 71° , you alter course to W.S.W., and continue thus for 440 leagues, until you touch the 60th degree, where you should find the Strait of Anian. In this manner you will accomplish the same navigation which I have made. . . .”

In fairness to Maldonado, and in order to dispose of a preliminary cavil—namely, that Bering Strait is not in lat. 60° N., but in approximately 66° N.—it may be as well to point out that in a later portion of the narrative Maldonado relates that he had been led to expect to find the strait in lat. 60° N. by an account of it which he had received from one Master John Martinez, a Portuguese pilot of considerable reputation; but that he actually fell in with it, coming from the northward, when he was more than a hundred leagues ($6^{\circ} 40'$) from that latitude—in which case the discrepancy in latitude disappears, although that between the two latitudes given in his narrative remains.*

His account of his strait is too long for quotation, but this differs so widely from Bering Strait that it is scarcely possible, at first, to take Amoretti seriously when he undertakes to prove the two identical. Such, however, is the fact. After reading all Amoretti's remarks, and his Appendix,† very carefully, I have formed the conviction that he was quite incapable of making a joke about any subject whatever.

According to Maldonado, his strait was sixty miles long, with a width of two miles at the northern entrance and a mile at the southern (where there was an islet, occupying the centre of the channel). Bering Strait, on the other hand, has a minimum width of some fifty-

* Elsewhere he speaks of the shores of the strait as being in 50° N.

† A “Lettera Apologetica,” addressed to one “Sig. B. di L.,” in Italian; the remainder of the text is in French.

five miles, while it is formed by two projecting headlands, and has very little "length" at all. The unhappy Amoretti, confronted with these facts, can only suggest that the MS. is corrupt, that volcanic action may have widened the strait since 1588, or that Maldonado meant only that the navigable channel was narrow!*

In his other details of the strait, Maldonado is no more fortunate.† He speaks of finding fruit trees, such as lychees and wild grapes: and of seeing not only partridges, hares, and wild deer (whose presence is possible, but improbable), but also two species of wild pigs! In fact, his whole account of the Bering Strait region represents it as the sort of place one would expect to find in 40° N. or thereabouts. It is true that, as Dr. Stefansson has repeatedly pointed out,‡ our notions of the Arctic as a place of eternal snow and gloom are entirely erroneous—for example, the summer visitor to Spitsbergen (80° N.) finds it carpeted with wild flowers and infested by myriads of mosquitoes—but Maldonado writes of the Arctic in the style of one who not only has never been there, but who has no real conception that its climate is noticeably different from that of his own Spain.

Anticipating de Fonte, he states that during a stay which he made, during April, May, and the beginning of June, in a port on the American side of the strait (which port Amoretti naturally identifies as Norton Sound) he saw, coming from the Pacific and making for the strait, a ship of some 800 tons. He took her

* Unfortunately, Maldonado's MS. contains some "views" of the strait which entirely negative this opinion.

† Except where he remarks that the tides in the strait are feeble—which happens to be the case.

‡ See, for example, the chapter "The North that never was," in his *Northward Course of Empire* (London, n.d. = 1922).

for a German, or a Russian, and believed that she was making for Archangel, along the North-East Passage. He managed to converse with her crew in Latin, but did not find them communicative; although he pays them the compliment of observing that "they appeared to be Christians—if not Catholics, at least Lutherans." He gathered that they had a consort in the neighbourhood. They soon sheered off, and he saw them no more. He began his homeward voyage not long afterwards.

Did he? The most natural conclusion to be drawn from his narrative is that it is either a deliberate lie, put forward to ingratiate himself with the Spanish authorities, or at best the result of prolonged brooding (verging upon monomania), and bearing about the same relation to actual truth as Abel Fosdyk's account of what happened on board the *Mary Celeste*.*

The assumption that his story was a deliberate lie removes many difficulties. On the other hand, a man of Maldonado's day who wished to recommend himself to his Government as an explorer would surely, one imagines, stand little chance of favourable notice if he could give no attested particulars of his former life and services, and relied entirely upon the narrative of an (imaginary) exploit, no one detail of which could be supported by the independent evidence of any other person whatsoever. And, if his story had been taken seriously, what did he expect to gain by it? The most natural course for Philip II

* Abel Fosdyk, a seaman turned gardener and odd-job man, left a MS. account, first published in *The Strand Magazine* for November 1913, of the circumstances surrounding the total disappearance of the crew of the *Mary Celeste*, a brigantine stated to have been picked up on the high seas on December 5th, 1872, in perfect order but with no soul on board. There seems to be little doubt that Fosdyk had brooded upon the subject until he persuaded himself that he had once been a member of the *Mary Celeste's* crew—much in the same way that advancing years and periodical surfeits of Curaçao combined to convince George IV that he had commanded a brigade at Waterloo.

to adopt would have been to act as Ferdinand did when Columbus came back in triumph from the West Indies; to send the discoverer out again, better equipped, to explore and consolidate his discoveries. If Maldonado had been ordered to repeat his (imaginary) voyage, his position would scarcely have been an enviable one: unless, indeed, his ignorance of official methods led him to suppose that his unsupported assertions, promptly accepted at their face value, would be rewarded with large sums of money "and no questions asked."* An honest monomaniac, on the other hand, who really believed that he had been to the Strait of Anian and could lead others to it, would not be troubled by any forebodings of ill success, disgrace, and exposure. It is not even necessary to suppose that Maldonado actually believed that he had really made his voyage of 1588. As seems to have been the case with Raleigh's last, ill-fated expedition in search of his Orinocan gold-mine, he may have been a firm believer in the existence of his strait, and have professed more knowledge of it than he possessed in order to obtain from the authorities the means of testing his belief.

Such a theory is more plausible than that of sheer imposture, for it provides a reasonable motive for Maldonado's actions, while it can still "pray in aid" the remarkable discrepancies between his story and the reports of contemporary Arctic explorers—discrepancies which, had he been a mere impostor, he would probably have been at pains to avoid.† Some of these have already

* I understand that the use of this formula, once so common in advertisements of lost property, is now held to be illegal (Larceny Act, 1861, Sec. 102).

† It is true that little could have been known in Spain, at the time of the Armada, of the early English N.W. voyages; but many Spanish seamen had had experience of the conditions obtaining around Newfoundland—which Maldonado asserts to have been his own point of departure.

been noted, but here are a few more. He speaks of sighting Friesland, which does not exist, and is believed to have been a geographical myth:* he must, by his own account, have passed (as no one else has ever succeeded in doing) through Hecla and Fury Strait, which is only about two miles across in its narrowest part, although by his account it should have been about eighty: and although he made his East-West passage in February and March he met, apparently, with no serious obstruction from ice. At such a time of the year, the channels of the Canadian Arctic Archipelago are now known to be completely blocked with ice, which does not break up sufficiently to allow of navigation until the late summer, if at all.

His return voyage is supposed to have been made in June and July, to which this objection does not apply; but his earlier passage can only be admitted by postulating that he was favoured with a miraculously clear and early season—such a season as the famous Antarctic summer of 1822–3, when the Weddell Sea, for the only time in its recorded history, was entirely clear of ice. Curiously enough, it is true that the Arctic summer of 1588 was an unusually “open” one. John Davis, who made his third Polar voyage in that year, found the “sea all open to the westward and northward” at his furthest north, 72° 12' N.; while, looking from Mount Raleigh (on the western side of Davis Strait) he could see no ice to the north, but what appeared to be a great sea, “free, large, very salt and blue, and of indeterminable depth.” But open water in Davis Strait in summer is a very different thing from open water in Prince Regent Inlet, or in Lancaster Sound, in March.

* As usual, Amoretti suggests that it has disappeared as the result of volcanic action. It is sometimes identified with the “sunken land of Bus,” reported by Frobisher and vainly searched for by Pickersgill in 1776.

And yet, to my mind, there is a quality about Maldonado's story which makes one loath to dismiss it as mere fable. I cannot hope to define this quality better than Mr. Hilaire Belloc has already done, when he speaks of

"... that exasperating quality for which we have no name, which certainly is not accuracy, and which is quite the opposite of judgment, yet which catches the mind as brambles do clothes, causing us continually to pause and swear."*

It must be remembered, in the first place, that the only account of Maldonado's voyage which has come down to us is a short account containing many clerical errors, and ostensibly drawn up as a memorandum addressed to persons who probably were not very conversant with navigation or geography. It does not profess to have the precision which one would expect in a ship's log or an explorer's journal; and part of it is undoubtedly mere propaganda. The rest may be only romance; but it is also possible that that romance may have a basis of fact. There are several narratives of accepted voyages to be found in Hakluyt, and elsewhere, which read little, if at all, more convincingly than Maldonado's.

The most curious feature of the *Relation* . . . is this. If one reads through the account of Maldonado's route with the assistance of modern charts, it is not difficult to lay it down, within the limits of vagueness which one usually tolerates when dealing with sixteenth-century voyages, in a way which makes his voyage possible, if wildly improbable (see Fig. 14). We can take him along Hudson Strait, northward up Fox Channel, through Prince Regent Inlet (assuming, after the manner

* *The Path to Rome*—in the opening disquisition on the Proverb-Maker.

of Amoretti, that he passed Hecla and Fury Strait in a fog) and so into Lancaster Sound, where he would, as he said, be in approximately lat. 75° N. Thence we can send him along Melville Sound, and south-westward past C. Barrow (the "high land" in 71° N.) along the American coast to Bering Strait.* Theoretically, he could have had open water almost all the way—that is to say, every part of this route, except a portion of Melville Sound,

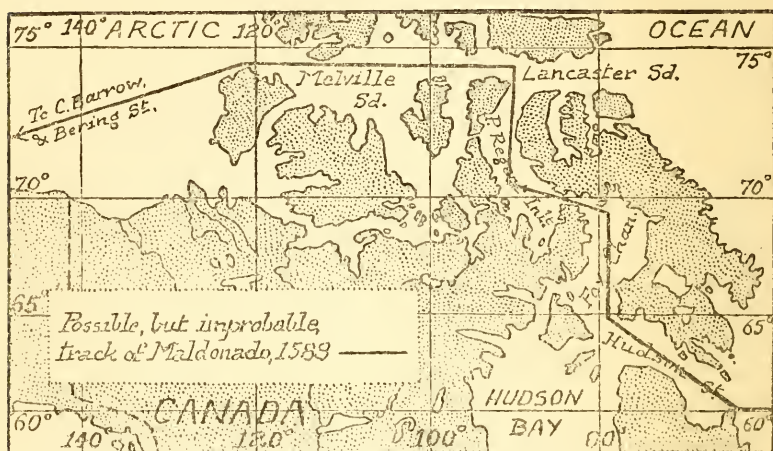


FIG. 14.—Maldonado's track, reconciled with a modern chart of the N.W. Passage.

has been known, *at times*, to be clear of ice. In all human probability, on the other hand, nine-tenths of it (on the average) are, at any given time, completely ice-blocked.

If Maldonado had been an arm-chair geographer with no Arctic experience, compiling a hoax with the assistance of modern charts, this is the sort of route he would have chosen. But at the time when he wrote most of it was entirely uncharted—the portion between (and including) Bering Strait and Prince Regent Inlet

* See Fig. 14.

entirely so. If his "strait of Labrador" was sheer guess-work, he was a good hand at guessing; and it is exceedingly curious that he should postulate the necessity of going as far northward as 75° , which necessity is in exact agreement with fact.* Surely, one thinks, if he were writing mere propaganda, designed to attract, he would have laid the route to his strait in lower and more favourable latitudes.

There is, in fact, a curious resemblance between the question raised by his voyage and that connected with the "Dauphin" maps of Australia. On these maps, most of which were drawn (so far as is known) between 1530 and 1570, the western, northern, and eastern coasts of Australia are shown in considerable detail, while very much exaggerated in size. No authority for such a charting is known; and it is a matter of opinion whether such maps are efforts of imagination, or (as has been contended) based upon forgotten Portuguese voyages.† The first ship which is definitely known to have sighted any part of Australia is the Dutch *Duyfhen*, in the spring of 1606; while the eastern coast remained entirely unknown until Cook explored it in 1770. In both cases—Maldonado's voyage and the "Dauphin" maps—I imagine that those who have unlimited faith in the possibilities of mere coincidence will dismiss the evidence as "obvious guess-work": while a few will consider that it may not be all guess-work—that it may have a substratum of real and forgotten achievement.

* Bellot Strait, dividing N. Somerset Island from the American mainland, has never been found sufficiently clear of ice for a ship to pass through it. In consequence, for navigational purposes the American continent may be regarded as extending to the north coast of N. Somerset, in lat. $74^{\circ} 11' N$.

† R. H. Major (*Early Voyages to Australia*) and G. Collingridge (*The Discovery of Australia*) accept the "Dauphin" maps as genuine records of discovery; the opposite view is most ably expounded in Professor Arnold Wood's *Discovery of Australia* (London, 1922).

It may be that Maldonado was an impostor—on the facts, it seems most probable that he was. He may have been a visionary enthusiast of a mentally-unstable type, unable to distinguish between fact and theory. Or, finally, he may have actually accomplished something—not, indeed, so much as he claimed, but some actual piece of Arctic exploration of which he could give no clear account—which would, if we knew more of it, entitle him to a place among the heroes of all nations who sought so long and so bravely for the North-West Passage.

I call them heroes, because to my mind there is something essentially heroic in all Polar exploration—not only in exploration of the purely scientific kind, which seeks knowledge for its own sake (and thus, ultimately, for the general good of humanity), but even in that of the commercial type, devoted chiefly to immediate profit. The Antarctic whalers of to-day are in the direct line of descent from the sealers of a century ago and the “merchant-venturers” of Elizabethan times—and while their gains may be greater, as their equipment is certainly superior, I do not think that in courage, resolution, and endurance they are one whit less remarkable. And if this be true of the men who, like Scoresby, Weddell, Biscoe, Larsen, and a hundred others, have made their way Pole-wards on the chance of earning a bare living—and, in a higher degree, of the men who have spent, and are spending, the best years of their lives in exploration directed to purely scientific ends—still more is it true of the men who sought for Franklin, who died with Scott, and who gave their lives to succour Nobile. Of them, and of all those who devote their manhood to that jealous and grudging mistress, Polar exploration, it can most truly be said that they have shaped their lives in

conformity with the heroic ideal so well expressed by Spenser* long ago:

*Is not short payne well borne that brings long ease
And lays the soul to sleep in quiet grave?
Sleepe after toyle, port after stormie seas,
Ease after warre, death after life does greatly please.*

* *The Faery Queene*, ix. 40.



THE LAST OF THE ALCHEMISTS

THE title of this essay is misleading. There have been alchemists in all ages, and there always will be: and by "alchemists" I do not mean fools or impostors, but men who have soberly and patiently attempted (and, in my judgment, occasionally accomplished) the transmutation of certain of the "base" metals, such as lead, silver, and mercury, into gold.

The subject is one which it is difficult to approach with an open mind—although that difficulty has been considerably lessened by the chemical and physical discoveries of this century. But in most people's minds the term "alchemist" is, I think, as surely and naturally associated with "impostor" as "succulent" with "bivalve," or "temporary" with "embarrassment." We know, or we knew until recently, that the chemical elements are immutable—such, we say, is a "Law of Nature," and since it does not conflict with our private inclinations we are under no temptation to infringe it, or to let others do so. In Science, as in religion, orthodoxy pays best, and the heretic questions accepted views at his peril. He must be prepared for a long and bitter fight—and, if in the long run he should succeed in extorting a grudging acquiescence from his opponents, he generally finds that his victory has been dearly bought. The dust of conflict besmirches the victor's crown. As has been truly said, public opinion on the subject of a

novel and heterodox point of view generally goes through three well-marked stages.

- (a) It is anti-scriptural.
- (b) (say ten years later.) At all events, it can never be of any practical value.
- (c) (after a similar interval). It has been well-known and accepted for a long time. Where is the originality in it?

And yet it is not in the least paradoxical to say that every advance in human knowledge has been made by men who were not content to take anything whatever on trust, but proceeded in strict accordance with the precept "Prove all things; hold fast to those that are good." It is perfectly true that, in science as in architecture, one cannot build durably without foundations; but it is equally true that these are the better for periodical inspection.

Macaulay, the apostle of mid-Victorian complacency, once wrote* (no doubt with general approval):

"There are branches of knowledge with respect to which the law of the human mind is progress. In mathematics, when once a proposition has been demonstrated, it is never afterwards contested. Every fresh story is as solid a basis for a new superstructure as the original foundation was. Here, therefore, there is a constant addition to the stock of truth. In the inductive sciences, again, the law is progress. Every day furnishes new facts, and thus brings theory nearer and nearer to perfection. There is no chance that either in the purely demonstrative, or in the purely experimental sciences, the world will ever go back or even

* Review of Von Ranke's *History of the Popes*, *Edinburgh Review*, October 1840.

remain stationary. Nobody ever heard of a reaction against Taylor's theorem* or of a reaction against Harvey's doctrine of the circulation of the blood."

If this passage illustrates anything except its author's proverbial "cocksureness,"† it is the extreme difficulty of recording dogmatic truth without providing amusement for succeeding generations. In passing, it may be stated that Macaulay's mathematical knowledge was so scanty that he was "gulfed" in the Cambridge Tripos of 1822; *i.e.* he obtained Honours in Classics, but failed in mathematics.

If Macaulay had wished to expand his argument by a geometrical illustration, he would probably have selected, as an example of a universally-accepted truth, Euclid's theorem‡ that the three interior angles of any plane triangle are together equal to two right angles; or Playfair's Axiom.§ These lie at the very roots of Euclid's system of geometry; a system which stood unquestioned for nearly two thousand years, and which Kant and his school regarded as an irrefragable proof of the existence of intuitive truth—truth, that is, apprehended independently of the senses, or of experience. Yet neither can be proved, and we know now that neither is necessarily true. Some years before Macaulay wrote the passage just quoted, the foundations of geometry had been, for the first time,|| independently examined

$$* f(x + z) = f(x) + zf'(x) + \frac{z^2}{2!}f''(x) + \dots$$

† "I wish," said Sydney Smith, "that I could be as cocksure of any one thing as Macaulay is of everything."

‡ *Euclid*, i. 32.

§ "Two intersecting straight lines cannot both be parallel to a third straight line."

|| In saying this, I am not forgetting the work of Saccheri, Lambert, Gauss, and Schweikart. But the latter two never published their investigations, and neither Saccheri nor Lambert could bring himself to accept the logical con-

and reconstructed by N. Lobachevski,* of Kasan, and János Bolyai,† of Maros-Vásárhely, in Hungary. Through their work, and that of their successors, such as G. B. F. Riemann, we know that there are other systems of geometry, free from the inconsistencies of Euclid's, in which the angles of a plane triangle may amount, in the aggregate, to more or less than two right angles; and that in such systems two‡ intersecting straight lines may both be parallel to a third. In consequence Geometry, long regarded as the crown of "pure mathematics," has now taken its rightful place as one of the experimental sciences.

It does not follow, then, because a point of view appears to be well-founded, and because it has been unquestioned for centuries, that it is necessarily correct. There is probably no popularly-received belief which is absolutely true; and certainly none whose foundations would not be the better for stringent re-examination at intervals. But in such matters the voice of authority is still far too prone to echo the fatuous saying of a mathematical writer well-known in his day, the Rev. Isaac Todhunter:

"... If he [the student] does not believe the statement of his teacher—probably a clergyman of mature knowledge, recognized ability, and blameless character—his suspicion is irrational, and manifests a want of power of appreciating evidence."

clusion to be drawn from their own results. Lobachevski and Bolyai are unquestionably the first two geometers to have the courage of their convictions upon this subject.

* *Geometrical Researches on the Theory of Parallels*, Berlin, 1840.

† *The Absolute Science of Space*—appendix to W. Bolyai's *Tentamen*, Maros Vásárhely, 1832–33.

‡ Or, none. In Riemann's system of non-Euclidean geometry no parallel lines exist.

It is human, I suppose, to prefer one's old *mumpsimus* to the new *sumpsimus*; but the result generally is that inconvenient truth seldom has a fair hearing. In the geometrical case just instanced, I doubt whether one boy in a hundred, when learning geometry, ever hears of the existence, or even the possibility, of the non-Euclidean systems. And, to take another example, how many clergymen are accustomed to tell their flocks the accepted results of modern Biblical criticism? How many of them even know those results?

It has come, rightly or wrongly, to be an accepted popular belief that alchemy—the transmutation of baser metals into gold—is, always has been, and always will be a physical impossibility. And yet it is strictly true that there is a considerable body of historical evidence pointing to the conclusion that such transmutation has, in the past, been occasionally effected—evidence of such weight that, did it relate to any more probable event, we should be compelled either to accept it or to cease putting any faith in recorded testimony.

It is unquestionably true that the modern prejudice against any serious discussion of alchemy arises, in great measure, from the general trickiness and chicanery of its professors. In what are loosely called the Middle Ages—in, that is to say, the infancy of scientific knowledge—an investigator whose conscience debarred him from entering the Church found it even harder to gain a living by purely scientific researches than is the case to-day. If he were inclined to astronomy, then in the intervals of his observations he cast horoscopes and “ruled the planets,” blessing his stars that, as Kepler remarked, “Nature, which has given to every animal the means of subsistence, has designed Astrology to be the support and handmaid of Astronomy.” And if his

tastes were chemical, his energies were directed, either by natural inclination or by the compulsion of necessity, towards alchemy. Here was something which even princes could understand. Just as Gladstone was attracted to one of Faraday's discoveries by the latter's assurance that it would soon be taxable, so the mediæval monarch, Prince, Markgraf or what-not readily afforded shelter and stipend to a professed alchemist for a period depending principally upon the length of his purse and the "cunning man's" adroitness in parrying inconvenient inquiries as to the progress of his labours. In those days the tide of opinion ran as strongly in favour of the possibility of transmutation as it has now set the other way; and one can scarcely blame the early chemists if, as the price of carrying on their researches, they encouraged—or, at least, did nothing to discourage—this idea. And even if they knew in their hearts that they possessed no method of accomplishing transmutation, that is not to say that they disbelieved in its theoretical possibility. The evidence is all the other way. It is undeniable that a chemist of, say, A.D. 1600 would have been far less astounded had he succeeded in transforming a bar of lead into one of gold than if he had combined hydrogen and oxygen to form water (then regarded as a typical element): or than if he had been shown that a poisonous metal and an equally poisonous gas were the sole constituents of one of the most necessary things in the world—common salt.

But in all ages rogues, as the Tichborne claimant acutely remarked,* have been on the look-out for their natural prey, of whom, according to the American

* "Some folk has money and no brains, some has brains and no money. Surely them as has money, no brains, was made for them as has brains, no money." (Arthur Orton, alias Thomas Castro, alias Sir Roger Charles Tichborne, Bart., alias C.33, Dartmoor—in a pocket-book found at Wagga-Wagga.)

estimate, one is born every second. And they found the credulous and embarrassed potentates of mediæval Europe a flock well worth shearing; while as the most convenient means of separating any given fool from his money they employed, in lieu of the three-card juggle, or the confidence trick beloved of "Patsy" and other practitioners in demand at Scotland Yard, the profession of alchemy. And if alchemy be defined simply as the "making" of gold, some of them were, for a limited time, quite successful.

Their methods were various. To quote Thomson's *History of Chemistry*:

"Sometimes they made use of crucibles with a false bottom; at the real bottom they put a quantity of oxide of gold or silver, this was covered with a portion of powdered crucible, glued together by a little gummed water or a little wax; the materials being put into this crucible and heat applied, the false bottom disappears, the oxide of gold or silver is reduced, and at the end of the process is found at the bottom of the crucible, and is considered as the product of the operation.

"Sometimes they make a hole in a piece of charcoal and fill it with oxide of gold or silver, and stop up the mouth with a little wax; or they soak charcoal in solutions of these metals; or they stir the mixtures in the crucible with hollow rods containing oxide of gold or silver within, and the bottom shut with wax:* by these means the gold or silver wanted is introduced during the process, and considered as a product of the operation.

* There is a vivid account of a transmutation of this kind, supposed to have been effected by one Galeotto in the presence (but not to the satisfaction) of Leonardo da Vinci, in Merejkowski's novel, *The Forerunner*.

“Sometimes they have a solution of silver in nitric acid, or of gold in aqua regia,* or an amalgam of gold or silver, which being adroitly introduced, furnishes the requisite quantity of metal. A common exhibition was to dip nails into a liquid, and take them out half converted into gold. The nails consisted of one-half gold, neatly soldered to the iron, and covered with something to conceal the colour, which the liquid removed. Sometimes they had metals one-half gold the other half silver, soldered together, and the gold side whitened with mercury; the gold half was dipped into the transmuting liquid and then the metal heated; the mercury was dissipated, and the gold half of the metal appeared.”

In view of the foregoing it may, perhaps, not be entirely a coincidence that the decline of belief in alchemy corresponds roughly, in point of date, with the appearance of the professional conjurer. Had Robert-Houdin or Buatier de Kolta lived in those piping times, and escaped (which seems unlikely) the penalties meted out to sorcerers, what a harvest they must have reaped!

But, just as in former days the Church of England numbered Titus Oates among her ministers along with the saintly Bishop Ken; and as in our own day the profession of letters embraces both myself and Mr. Edgar Wallace; so among the mediæval alchemists and their successors are to be found not only many charlatans of the Cagliostro type, but also men of high ideals and scrupulous honesty—such men as Van Helmont, Helvetius, and Robert Boyle, all three of whom, as will shortly appear, have testified to having personally effected transmutation in their own laboratories, albeit

* A mixture of nitric and sulphuric acids.

under somewhat peculiar circumstances. I have selected these three cases because of the eminence of the principal actors, but I could easily give many more; while a list of the eminent men who have affirmed their belief in the possibility of transmutation would also include the names of Sir Isaac Newton, Leibnitz, Sir Humphry Davy, and (in our own day) Sir William Crookes and others. Newton spent a considerable time in alchemical experiments, and left several (unpublished) MSS. on the subject—Leibnitz acted for some time as secretary of a German alchemical society—Davy declared that he could not stigmatize the doctrines of the alchemists as “unphilosophical”—and Crookes, as will be seen, constructed a “force-engine” for the purpose of transforming silver into gold. I may mention, in passing, that the student of the subject will find a very full and accurate account of the historical evidence for transmutation in Schmieder’s *Geschichte der Alchemie*,* if he can obtain it.

Van Helmont.

Jean Baptiste van Helmont (1577–1644), a Belgian, is generally regarded as the greatest chemist of the seventeenth century. It will interest politicians to know that he invented the term “gas.” That he was no friend of the professed alchemists of his day is shown by his referring to them as “a diabolical crew of gold and silver sucking flies and leeches.” Yet here is his account† of a transmutation which he himself performed:

“... For truly, I have divers times seen it (the philosopher’s stone) and handled it with my hands:

* *Geschichte der Alchemie*, Karl Christoph Schmieder (Doctor der Philosophie und Professor zu Kassel). Halle, 1832.

† In his *Oriatrike* (translated by one J. C.). London, 1662.

but it was of colour, such as is in Saffron in its powder, yet weighty, and shining like powdered glass: There was once given unto me one-fourth part of one grain: But I call a grain the six hundredth part of one Ounce: This quarter of one Grain therefore, being rouled up in Paper, I projected* upon eight Ounces of Quick-silver made hot in a Crucible; and straightway all the Quick-silver, with a certain degree of Noise, stood still from flowing, and being congealed, settled like unto a yellow Lump: but after pouring it out, the Bellows blowing, there were found eight Ounces, and a little less than eleven Grains of the purest Gold. Therefore one only Grain of that Powder, had trans-changed 19,186 † Parts of Quick-silver, equal to itself, into the best Gold. . . .”

Everywhere, Van Helmont says that he was given the “stone” by a stranger, “a friend of one evening’s acquaintance.” Figuiet, in his *L’Alchimie*, says that Van Helmont came by it in 1618, in his laboratory at Vilvorde, near Brussels. He gives a faithful version of Van Helmont’s narrative, but finds himself compelled to explain it away; and, *faute de mieux*, has perforce to suggest that some emissary of the mysterious “stranger” had craftily substituted a trick crucible of the pattern already described—or, failing this, that the mercury employed was, actually, an amalgam of mercury and gold. How a practised chemist like Van Helmont (whose personal honesty has never been impugned) could have failed to detect the substitution, or to perceive the

* *I.e.*, placed.

† There is some arithmetical blunder here. If, as seems most likely, he means by “Eight Ounces and a little less than eleven Grains” 8 oz. less 11 gr., the proportion would be as 19,156 to 1: if he actually obtained $10\frac{3}{4}$ grs. more matter from the crucible than he put into it, the proportion would be as 19,244 to 1.

residual mercury (on the second hypothesis) he does not attempt to explain. He continues:

“Van Helmont . . . became an avowed partisan of alchemy. In honour of the incident he named his newly-born son Mercurius.* This Mercurius Van Helmont bore out his alchemical baptism; he converted Leibnitz to the view; during his whole life he sought for the philosopher’s stone, and though he never found it, he died a fervent believer in its existence.”

Helvetius.

Johann Frederic Helvetius, physician to the Prince of Orange, a man of eminence in his profession who also stood very high as a chemist, published in 1667 a work† giving an account of a transmutation much resembling that described by Van Helmont. Here are some extracts from the rather copious original:

“On the 27 December, 1666, in the forenoon, there came to my house a certain man, who was a complete stranger to me, but of an honest, grave countenance, and an authoritative mien, clothed in a simple garb. . . .

“After we had exchanged salutations, he asked me whether he might have some conversation with me. . . . After some further conversation, the Artist Elias (for it was he) thus addressed me.

“ ‘Since you have read so much in the works of the Alchemists about this Stone, its substance, its colour,

* Francis Mercurius Van Helmont, 1618–91.

† *Brief of the Golden Calf; Discovering the Rarest Miracle in Nature; how by the smallest piece of the Philosopher’s Stone, a great piece of common lead was totally transmuted into the purest transplendent gold, at the Hague in 1666* (Translation). The actual date of the transmutation appears to have been January 19, 1667.

and its wonderful effects, may I be allowed the question, whether you have not yourself prepared it?" On my answering his question in the negative, he took out of his bag a cunningly-worked ivory box, in which there were three large pieces of a substance resembling glass, or pale sulphur, and informed me that here was enough of the Tincture for the production of twenty tons of gold. When I had held the precious treasure in my hand for a quarter of an hour (during which time I listened to a recital of its wonderful curative properties) I was compelled to restore it to its owner, which I could not help doing with a certain degree of reluctance. After thanking him for his kindness in showing it to me, I then asked him how it was that his Stone did not display that ruby colour, which I had been taught to regard as characteristic of the Philosopher's Stone. He replied that the colour made no difference, and that the substance was sufficiently mature for all practical purposes.

"My request that he would give me a piece of his Stone (though it were no larger than a coriander seed) he somewhat brusquely refused, adding, in a milder tone, that he could not give it to me for all the wealth I possessed, and that not on account of its great preciousness, but for some other reason that it was not lawful for him to divulge. . . .

"When my strange visitor had concluded his narrative, I besought him to give me a proof of his assertion, by performing the transmutatory operation on some metals in my presence. He answered evasively, that he could not do so then, but that he would return in three weeks, and that, if he were then at liberty to do so, he would show me something that would make me open my eyes.

"He appeared punctually to the promised day, and invited me to take a walk with him. . . . At last I asked him point-blank to show me the transmutation of metals. I besought him to come and dine with me, and to spend the night at my house; I entreated; I expostulated; but in vain. He remained firm. I reminded him of his promise. He retorted that his promise had been conditional upon his being permitted to reveal the secret to me. At last, however, I prevailed upon him to give me a piece of his precious Stone—a piece no larger than a grain of rape seed. He delivered it to me as if it were the most princely donation in the world.

"Upon my uttering a doubt whether it would be sufficient to tinge more than four grains of lead, he eagerly demanded it back. I complied, in the hope that he would exchange it for a larger piece; instead of which he divided it in two with his thumb, threw away one half, and gave me back the other, saying: 'Even now it is sufficient for you.' Then I was still more heavily disappointed, as I could not believe that anything could be done with so small a particle of the Medecine.

"He, however, bade me take two drachms, or half an ounce, of lead, or even a little more, and to melt it in the crucible; for the Medecine would certainly not tinge more of the base metal than it was sufficient for. I answered that I could not believe that so small a quantity of Tincture could transform so large a mass of lead. But I had to be satisfied with what he had given me, and my chief difficulty was about the application of the Tincture.

"I confessed that when I held his ivory box in my hand, I had managed to extract a few crumbs of his

Stone, but that they had changed my lead, not into gold, but only into glass. He laughed, and said that I was more expert at theft than at the application of the Tincture. 'You should have protected your spoil with yellow wax, then it would have been able to penetrate the lead and transform it into gold.' . . .

"With a promise to return at nine o'clock the next morning, he left me. But at the stated hour on the following day he did not make his appearance; in his stead, however, there came, a few hours later, a stranger, who told me that his friend the Artist was unavoidably detained, but that he would call at three o'clock in the afternoon. The afternoon came; I waited for him till half past seven o'clock. He did not appear.

"Thereupon my wife came and tempted me* to try the transmutation myself. I determined, however, to wait till the morrow, and in the meantime, ordered my son to light the fire, as I was now almost sure that he was an impostor.† On the morrow, however, I thought that I might at least make an experiment with the piece of Tincture which I had received; if it turned out a failure, in spite of my following his directions closely, I might then be quite certain that my visitor had been a mere pretender to a knowledge of this Art.

"So I asked my wife to put the Tincture in wax, and I myself, in the meantime, prepared six drachms of lead; I then cast the Tincture, enveloped as it was in wax, on the lead; as soon as it was melted, there was a hissing sound and a slight effervescence, and

* Whether her name was Eve, is not stated.

† This is obscure. I do not imagine, however, that Helvetius contemplated offering Elias, should he return, any personal violence, or making him the subject of an *auto da fè*—he is probably speaking of the fire in his laboratory furnace.

after a quarter of an hour I found that the whole mass of lead had been turned into the finest gold. Before this transmutation took place, the compound became intensely green, but as soon as I had poured it into the melting pot it assumed a hue like blood. When it cooled, it glittered and shone like gold. We immediately took it to the goldsmith, who at once declared it to be the finest gold he had ever seen, and he offered to pay fifty florins an ounce for it.

"The rumour, of course, spread at once like wild-fire through the whole city; and in the afternoon I had visits from many illustrious students of this Art; I also received a call from the Master of the Mint and some other gentlemen, who requested me to place at their disposal a small piece of the gold, in order that they might subject it to the usual tests. I consented, and we betook ourselves to the house of a certain silversmith, named Brechtil, who submitted a small piece of my gold to the test called the 'fourth': three or four parts of silver are melted in the crucible with one part of gold, and then beaten out into thin plates, upon which some strong *aqua fortis** is poured. The usual result of this experiment is that the silver is dissolved, while the gold sinks to the bottom in the shape of a black powder, and after the *aqua fortis* has been poured off, melted again in the crucible, resumes its former shape. . . .

"When we now performed this experiment, we thought at first that one-half of the gold had evaporated; but afterwards we found that this was not the case, but that, on the contrary, two scruples of the silver had undergone a change into gold.

"Then we tried another test, *viz.* that which is

* Nitric acid.

performed by means of a septuple of Antimony; at first it seemed as if eight grains of the gold had been lost, but afterwards, not only had two scruples of the silver been converted into gold, but the silver itself was greatly improved both in quality and malleability. Thrice I performed this infallible test, discovering that every drachm of gold produced an increase of a scruple of gold. . . .

"The gold I still retain in my possession, but I cannot tell you what has become of the Artist Elias. Before he left me, on the last day of our friendly intercourse, he told me that he was on the point of undertaking a journey to the Holy Land. . . ."

This strange story does not rest on Helvetius' unsupported testimony alone. Here is an excellent piece of corroborative evidence, in a letter written by one of the most upright men who ever lived—Baruch Spinoza the philosopher.

(Spinoza to Jarrig Jellis.)

" . . . The Helvetius matter having been mentioned by me to Voss, he laughed at me, and was surprised to find me occupying myself about such nonsense. In order, therefore, to clear up the business I went to the assayer Brechtel, who had tested the gold. He assured me that when the metal was fused he had added some silver, with the result that the quantity of gold had become augmented. This shows that there must have been something uncommon about the gold, seeing that it had the power of transmuting some of the silver into additional gold.

And not only Brechtel, but other persons who had been present at the test informed me that such

were the facts of the case. I afterwards went to Helvetius himself, who showed me the gold and the crucible with a little gold still sticking to it. He told me he had projected upon the fused lead barely a quarter of a grain of the philosopher's stone. He added that he would tell everybody of the circumstance. It seems that the same alchemist has done something similar in Amsterdam, where he may perhaps be found. This is all I have learned respecting the affair. . . ."

Helvetius, it will be noticed, speaks of "the Artist Elias" as if his name were well known. It may have been, at the time; but I have not succeeded in tracing him—at least, under that name. The speculative may, perhaps, amuse themselves in identifying him with John Buttadeus, alias Ahasuerus (otherwise styled "the Wandering Jew"), or with the "deathless" Count St. Germain who cured Louis XV's flawed diamond, or with the stranger who gave similar portions of his "Stone" to Van Helmont and, possibly, to Boyle. Like almost every alchemist in history, there is an irritating flavour of mystery and superior knowledge in what we hear of him; but it must be conceded that Helvetius' behaviour was not exactly such as to encourage Elias to put much confidence in a man who was not above confessing that he had stolen and wasted a valuable piece of property. On the other hand, we must allow that, to an ardent student of alchemy, the temptation offered by Elias's "Stone" was extreme; and in similar circumstances greater men than Helvetius have fallen. Pope Innocent X, in his early days, was kicked out of a French artist's studio, its enraged owner having caught him in the act of purloining a book which he especially coveted.

Boyle.

The Hon. Robert Boyle (1627-91) is generally regarded as the founder of modern chemistry, a distinction which he deservedly won by a long life of single-minded devotion to science. The amount of chemical and physical spade-work which he accomplished, and of which an account is given in the five folio volumes of his collected works, is almost incredible. By his rejection of all fanciful theories, and his patient accumulation of facts and observations, he showed himself possessed of a scientific mind in the true and only sense of the term. It is not a little remarkable, therefore, that he continued all through his long life to believe in the possibility of transmutation; an opinion which led him, in 1689, to procure the repeal of the Statute V Henry IV, c. iv, which decreed heavy penalties against any person convicted of "multiplying gold."* He had already published (in 1678) a most singular pamphlet entitled, *Of a Degradation of Gold . . .*† in which he gave a narrative of an experiment, performed by himself, which bears a curious resemblance to the experiences of Van Helmont and Helvetius. It may be added that a paper of his, entitled "Statement Concerning the Incalescence of Quicksilver with Gold,"‡ influenced several of Newton's alchemical experiments.§

Although there is little doubt that the pamphlet is intended to describe Boyle's actual experience, this is

* Repealed by I William and Mary. St. I, c. 30.

† *Of a Degradation of Gold, made by an Anti-Elixir: a Strange Chemical Narrative* (Anonymous). London: Printed by T. N. for Henry Herringman. (n.d. = 1678.)

This pamphlet was reprinted, practically unaltered, after an interval of no less than sixty-one years—in 1739. In this edition, Boyle's name appears on the title-page as the author.

‡ *Philosophical Transactions*, 21. 2. 1676.

§ See his letter to Oldenburg, in Boyle's *Works*, v. 396.

shrouded in a good deal of mystery. Not only is the work anonymous, but there is a preliminary advertisement, headed "The Publisher to the Reader," in which the former invites the latter to believe that he was allowed to peruse the MS. at his lodging, and proceeded to give it to the world without the trifling formality of asking its author's permission. Such may have been the case, for in those days an author was classed as *feræ naturæ*, with every man's hand against him, and no real copyright or other protection. But I think that in this case the "Advertisement" is simply a gentle mystification, written by Boyle himself, and not seriously intended to deceive.

The narrative is tedious and verbose. It is supposed to be told by one "Pyrophilus" (Boyle) to a company of fellow-chemists, afflicted with such names as "Simplicius," "Aristander," "Crattipus," and "Heliodorus." Subject to occasional interruptions Pyrophilus tells, in substance, the following tale.

Being in London, he called one day on "an ingenious Foreigner" who had visited him occasionally, in order to return a call. While there, a stranger, whom he had only once seen, came to visit his host, and Boyle and he got talking. The stranger had been to the East, and was about to go back there. Boyle asked him whether he had met any Oriental chemists, and was told he had encountered some as skilful as any in Europe, but fewer in number, and secretive as to their accomplishments. Civilities were exchanged,

"... and before he left the Town to go aboard the Ship he was to overtake, he in a very obliging way put into my hands at parting a little piece of paper, folded up; which he said contained all that he had

left of a rarity he had received from an Eastern *Virtuoso*, and which he intimated would give me occasion both to Remember him, and to exercise my thoughts in uncommon Speculations."

Having obtained some hints as to the experiment he was to attempt, Boyle procured the services of a "Doctor of Physick"* to assist him. The paper was found to contain a tiny quantity of powder—so small in amount that they could scarcely determine its colour, which they judged to be dark red. Its weight was estimated at one-eighth to one-tenth of a grain.

Weighing out two drachms of refined gold, they fused this without a flux and added the powder, maintaining the fusion for about a quarter of an hour longer. On allowing the crucible to cool, they found that while the gold had not lost any weight it was much altered in appearance, looking dirty, and being coated with a substance like "half-vitrified litharge."† Adhering to the side of the crucible was a globule, apparently of silver; while its bottom was covered with a glassy substance, partly clear yellow and partly deep brown, in which were five or six similar globules.

They tried the "debased gold" in various ways. Tested on a touchstone, it showed more like silver than gold. It was brittle—almost as brittle as bell-metal.‡ It could be cupelled, but with difficulty. They did not test it with nitric acid, having none handy.§ They determined

* The company are represented as being loud in their commendation of this prudent precaution.

† Lead oxide— PbO .

‡ Approximately three parts of copper to one of tin. As anyone knows who has listened to "Big Ben" (which was intended by Lord Grimthorpe, its designer, to be 22 copper, 7 tin, but was badly cast), bell-metal is easily fractured.

§ Nitric acid dissolves silver, and most other metals, but does not affect gold. Hence the joy of the *Puer Hebraeus* at being able to inform his astounded parent that he had been swindled, since his offspring had "tested der goldfish mit acid, and dey are not gold at all!"

its specific gravity, *à la* Archimedes, to be about $15\frac{2}{3}$ to 1 (that of gold is about 19, that of silver about $10\frac{1}{2}$). Boyle drew the conclusion that this powder was capable of altering the malleability, homogeneity, and specific gravity of "near a thousand times" its weight of gold.

The commercial use of such a powder is not apparent; but the experiment was certainly a very remarkable one. And Boyle closes his narrative with a dark hint of further wonders. "I have not (because I must not do it) as yet acquainted you with the strangest Effect of our admirable Powder."

It may be as well to point out that this experiment was made, and the account of it published, when Boyle was in the prime of life, or nearly so. I emphasize this, because some of his later writings indicate the gradual growth in him of a rather unscientific credulity. This is strongly marked in his last work,* published in the year of his death. In an appendix he gives, admittedly at second and remoter hands, a small collection of "Strange Reports," which he presumably published because he believed them. Strange they certainly are. We read of a piece of malleable red glass, owned by a "Monsieur P——r"; of a liquid (distilled from bismuth) that rises and falls in its vessel as the Moon waxes and wanes; of yet another "Stranger," believed to be an "Adeptus," who showed a very learned and experienced Physician "a runing (*sic*) Mercury of a lively Green"; and of a number of swallows dug out of the ice in Prussia and subsequently resuscitated. However, we must remember, in the last case, that even a sound naturalist like White of Selborne was doubtful whether swallows did not

* *Experimenta & Observationes Physicæ*, London, 1691.

hibernate, while to Johnson this was quite an article of faith.*

Although occurring at widely different times and places,† the events narrated by Van Helmont, Helvetius, and Boyle are so closely similar that they could almost be reduced to a common formula. In each case the story is told by a man of reputation and authority, who has effected transmutation by means of a very small quantity of a substance (more or less uniformly described) given to him by a mysterious and vagrant stranger, who thereupon disappears. The transmuting agent is expended, and none therefore remains for analysis. The principal actor publishes a statement of the events.

As already remarked, it would be easy to adduce many similar narratives. But those already instanced are sufficient, I think, to serve the purpose of an introduction to the very singular facts surrounding the tragic death of Dr. James Price of Guildford, commonly termed "The Last of the Alchemists."

Dr. James Price, F.R.S.‡

Price's style and title may, perhaps, conjure up a mental picture of someone not unlike the Elizabethan Dr. Dee, or Sidrophel in *Hudibras*—an old man with a fur gown and a nanny-goat beard, sitting in a room full of cabalistic books and curious instruments, with a skeleton grinning in the corner and a stuffed crocodile slung overhead. Actually, he was a brilliant young man of independent fortune, a Fellow of the Royal Society

* "He seemed pleased to talk of natural philosophy. . . . 'Swallows certainly sleep all the winter. A number of them conglobulate together by flying round and round, and then all in a heap throw themselves under water, and lie in the bed of a river.'" Boswell's *Life*, anno 1768.

† Brussels, 1618; Hague, 1666; London, about 1677.

‡ See Plate IV.



PLATE IV

DR. JAMES PRICE, F.R.S.

From a portrait in pastel by John Russell, R.A.

*Reproduced by courtesy of
The National Portrait Gallery*

[Facing page 200

and an honorary M.D. of Oxford. At the age of thirty he seemed to have the ball at his feet—to be one of fortune's favourites. He had something more than a competence: he had powerful friends, he had scientific tastes and the means of gratifying them, he had won honourable distinctions and he might reasonably hope to win more. He stood as high, in the early years of manhood, as many who count themselves fortunate in reaching such a position only at the close of their lives. And yet within a few months he was dead by his own hand, leaving his whilom friends and admirers to decide, if they could, whether he were an impostor or a madman. It is my own conviction that he was neither, but it must be remembered that no adequate biography of him is extant, and that the facts have to be put together piecemeal, after disentangling them as far as possible from a nexus of prejudice and exaggeration. Of contemporary information we have very little—and of first-hand evidence practically nothing except a pamphlet published by Price himself. These sources, with a certain amount of additional matter (chiefly obtained by re-working the authorities cited in the *D.N.B.* article on Price), have furnished the main outlines of the following sketch.

James Higginbottom,* afterwards James Price, was born in London in 1752.† Entering Magdalen Hall, Oxford, as a gentleman commoner, he matriculated there on April 15, 1777, and subsequently “proceeded M.A.” (November 21, 1777). Early in 1781 his maternal uncle, James Price of London, died, leaving to his nephew a

* This name is so spelled in his certificate recommending him for admission to the Royal Society. The *D.N.B.* and Brayley's *History of Surrey* both have “Higginbotham.” The point is not of fundamental importance.

† He was the son of James Higginbottom by Margaret his wife, sister of James Price.

fortune of some £130 per annum from real estate, and £10,000 to £12,000 in the funds. Under the terms of the bequest, Higginbottom adopted his uncle's name, and became James Price; and, determining to devote his life to chemical research, he purchased a small estate at Stoke, near Guildford, where he fitted up a laboratory in a beautiful, rambling house * which had previously been inhabited by another "character," one Dr. Irish.

Price seems to have possessed a natural bent for chemistry; and this, coupled with the improvement in his circumstances, pointed him out as a very fit person to be proposed for election into the Royal Society. It may be noted that in the early years of George III's reign the portals of that august body were not so closely guarded as they are to-day. Under the energetic presidency of Sir Joseph Banks (who ruled over it from 1778 until his death in 1820) much, however, had been done to debar the entry of candidates of the entirely useless and unscientific type so cordially welcomed by a former president, the amiable numismatist Martin Folkes.† Banks set his face—which, judging by his portraits, was well adapted to express scorn and contempt—sternly against the dilettante, the "distinguished foreigner," and the sprig of nobility; and it is a testimony to Price's merits and accomplishments that so young a man (he was elected F.R.S. at twenty-nine), little known outside his own circle, and with nothing of his work yet published, should have made his way into the charmed circle which Banks had been at such pains to delineate.

* My friend Dr. G. C. Williamson, of Guildford, informs me that Price's house was only pulled down about two years ago.

† The Royal Society, in Folkes' time, once came within an ace of adding to their number a gentleman whose only claim to scientific honours lay in his invention of a form of water-closet.

The certificate recommending him for election runs as follows:

“James ^{Price} ~~Higginbottom~~ of Magdalen Hall Oxford
M.A. A Gentleman well versed in various branches of
Natural Philosophy & particularly in Chymistry being
desirous of becoming a Member of this Society we
whose names are here underwritten do from our per-
sonal knowledge recommend him as a person very
worthy of that honour & likely to become a very
Usefull Member

(*Endorsed*)

Read Feby 8. 1781

1 15

2 22

3 March 1

4 8

5 15

6 22

7 29

8 April 5

9 26

10 May 3

Ballotted for and Elected

May 10, 1781.

R. Kirwan

S. Hemming

Geo. Atwood

R^d. Hy. Alexr. Bennet

Richard Brocklesby

R. Barker

Wm. Seward

Nevil Maskelyne

J. Lockman

Willm. Wright

Dan Solander.”

One or two of his proposers were a little above the rank and file of the Society. Atwood is still remembered by “Atwood’s Machine” for diluting and measuring the acceleration of falling bodies; Maskelyne was Astronomer Royal, and had not yet initiated that revolt against Banks’ authority in which he was to be so signally worsted; and Solander had sailed round the world with Banks and

Cook in the *Endeavour*, and was a close friend of the former.

During 1781, Price busied himself in experiments and preparations which gradually assumed a definitely alchemical trend. He left, so far as I can trace, no notes or MS. which were examined by competent chemists, and the sole source of information for his work is the pamphlet* which he published in 1782—a source whose reliability must stand or fall with the estimate which one forms of his whole character. If it be assumed that he was insane when he took his life, it is a fair inference that he was equally or nearly so when he wrote it, and when he made his public experiments: and if, on the other hand, it is considered that his experiments were deliberately fraudulent, his pamphlet will fall to be classed as a piece of propaganda directed to the same end. In what follows I have assumed that he did his best to set down an account of the truth—but, as will be seen, not the whole truth.

Here is his own account of the lines on which his work was planned.

“A frequent perusal of ancient chemical writers, and an early attachment to the metallurgic branches of Chemistry, inclined the author of the ensuing narrative to believe that the wonders related in books at present little read, though frequently exaggerated, had at least some foundation. The phænomena which he continually met with in the pursuit of his experimental enquiries contributed greatly to strengthen this opinion: He found also, that some discoveries

* An / Account / of some / Experiments / on / Mercury, / Silver and Gold, / Made at Guildford in May, 1782. / In the Laboratory of / James Price, M.D. F.R.S. / to which is prefixed / An Abridgment of Boyle's Account / of a degradation of Gold. / . . .

supposed to be modern, were really recorded in very ancient writers; but in terms so obscure, that the fact must rather be applied to explain the description, than the description to illustrate the fact.

"The positions of the Spagyric Philosophers * respecting metals, seemed to be very easily reconciled with the notions of more modern chemists. . . .

". . . Of their Earths, most have allowed the diversity; but specific gravity being usually considered as the least dubious mark of real identity between two bodies otherwise dissimilar, it seemed probable that Mercury and Gold had a basis nearly alike. . . .†

"The remarkable analogies between the habitudes of Silver and Mercury, to chemical solvents and other agents, are known to every Chemist.

"These, and a thousand other analogies, too obvious as well as too minute to relate, occurred in a course of incessant experiment, in which an ardent curiosity involved the author at a very early period. . . . Among this matter of unformed matter, where opinion fought with opinion, and *Chaos judged the strife*, the specious glitter of some broken gems, allured him to prosecute his search, and, if possible, dive to the bottom; the turbid stream did not permit his view of its utmost depths, but he returns to shew that he has been below the surface, and not quite in vain.

"To the Chemist it is unnecessary to hint at more analogies; to others it would be useless. . . ."

In other words, Price, as the result of studying the writings of the old alchemists in the light of his own

* Believers in the transmutability of metals. The term was coined by Paracelsus.

† Their approximate specific gravities are: gold, 19.5; mercury, 13.6 (lead, 11.4; silver, 10.5). It is curious that Price does not mention lead, which is denser than silver. The modern atomic weights are still closer: gold, 197.2; mercury, 200.0; lead, 206.4.

chemical knowledge, had come round to their doctrine that the heavier metals, such as gold, silver, and mercury, were really variant forms of the same substance; the doctrine enunciated, for example, by "Eirenaeus Philalethes":*

"... All metallic seed is the seed of gold; for gold is the intention of Nature with regard to all metals. If the base metals are not gold, it is only through some accidental hindrance; they are all potentially gold."

Some of the alchemists, indeed, went further, and regarded all the so-called "elements" as different disguises assumed by one elemental substance; corresponding, in modern times, to the "urstoff" of Hinrichs or the "protyle" of Crookes.

Having accepted this theory Price proceeded, strictly following the alchemical tradition, to search for some ingredient which, if added in small quantity to certain of the "base metals" under favourable conditions of heat and fluidity, would convert them, wholly or in part, into the precious metals—gold and silver. That such an ingredient (the so-called "Philosopher's Stone") actually existed was a widely accepted belief; and if, as seems likely, he knew the narratives of Van Helmont, Helvetius, and Boyle, he must have had at least an inkling of its probable character and of the technique required for its employment. In his reading, moreover, he must have encountered a large number of alchemical recipes; which, by reason of their obscure language, could, by his plan of interpreting them in the light of his own knowledge, be made to square with almost any formula he might select for experiment.

* In *The Metamorphosis of Metals* (see *The Hermetic Museum*, Vol. II, p. 19). The identity of this writer is not known with certainty. It seems most probable that he was one George Starkey (ob. 1665).

In the spring of 1782, Price believed himself to be in possession of small quantities of two ingredients, one capable of transforming mercury into silver, and the other silver or mercury, indifferently, into gold. These took the form of powders, the former white and the latter red. As to their composition, apart from the fact that they contained arsenic, nothing is known. According to Price, their preparation involved "a process tedious and operose," and, moreover, injurious to health. At the beginning of May 1782 he possessed about sixteen grains of the white powder and five of the red, all of which he expended in the once-celebrated experiments which he performed at Guildford between May 6th and 28th in that year.

I say "experiments," since that was what Price himself called them: but I am certain that he used the term as the conjurer does who tells you that he is about to perform "a few experiments in the art of legerdemain or sleight-of-hand."* It is simply unthinkable that a man with a reputation to lose would invite witnesses to see him perform an important and novel experiment which he had never attempted before. He must have first satisfied himself that his powders were capable of effecting transmutation; afterwards he expended what remained of them in repeating his results before some of his neighbours and a few persons of note who had come from farther afield.

Naturally, but somewhat unfortunately, he decided to give his demonstrations in his own laboratory. Naturally, because there he was working with familiar appliances and extensive resources; he avoided, for example, handicapping himself by undertaking the management of a

* Most of the best modern illusionists, of course, have discarded this sort of traditional hocus-pocus.

strange furnace. Unfortunately, because he thereby furnished his detractors with two excellent arguments. A man desirous of committing a fraud would naturally prefer to do so in a place under his sole control, and one whose distance from London militated against the attendance of witnesses possessing an inconvenient amount of expert knowledge. Price himself, as will be seen, thought that some explanation of the latter circumstance was necessary.

For the first experiment (made on May 6, 1782) he had only four witnesses, all neighbours; but rumours of his doings spread like wildfire, and he was soon able to command the attendance of as many selected persons as could be got into his laboratory. For example, the seventh experiment (May 25, 1782) was attended by:

Lord King

Lord Onslow

Lord Palmerston, F.R.S.

Sir R. Barker, Bart.

Sir Philip N. Clarke, Bart.

Rev. O. Manning, F.R.S.

*Rev. B. Anderson

Rev. G. Pollen

Rev. J. Robinson

Dr. Spence

W. Mann Godschall, F.R.S.

W. Godschall, Jun.

Wm. Smith, Esq.

— Gregory, Esq.

†F. Russell, F.R.S.

* An amateur chemist living near Guildford.

† This identification is not quite certain. Price speaks of him only as "Mr. Russell, a magistrate of the Place." Apparently, he knew enough of metallurgy to be able to make an assay.

Price's experiments were of three kinds:

- (a) Fusing mercury, and transforming a small portion of it into silver (with the white powder) or gold (with the red powder).
- (b) Fusing silver, and obtaining, by the red powder, an alloy of eight silver to one gold.
- (c) Forming an amalgam of mercury with either powder, resulting in the production of a small quantity of silver or gold.

The following account of the first experiment is quoted verbatim (stops included) as giving a fair example both of his results and methods, and of his style.

EXPERIMENT I

"Made May the 6th, 1782, before the Rev. Mr. Anderson, Capt. Francis Grose;* Mr. Russell, and Ensign D. Grose. The Gentlemen mentioned in the Introduction as the most proper witnesses of the process, then resident in the Town.

"Half an ounce of Mercury provided by Capt. Grose (bought at an apothecary's of the town) was placed in a small hessian crucible, brought by Mr. Russell on a flux composed of Borax, (also brought by him) a small piece of charcoal, taken out of a scuttle (fortuitously) by Mr. D. Grose and examined by the rest of the company, and a small piece of Nitre also taken out without selection, by the Rev. Mr. Anderson, from a quantity in common use, in the Laboratory; these being pounded together in a

* A well-known antiquary, celebrated both for his learning and for his Falstaffian bulk and humour. His *Classical Dictionary of the Vulgar Tongue* and his *Olio* may confidently be recommended to certain ultra-frank lady novelists—they have a fescennine flavour which even eighteenth-century readers found a little disquieting.

mortar which all the company had previously inspected, were pressed down into the crucible with a small pestle: on this flux the mercury was poured by Mr. Anderson, and upon it half a Grain, carefully weighed out by Mr. Russell, of a certain powder, of a deep red colour, furnished by Dr. P. was put on it by Mr. Anderson.

“The crucible was then placed in a fire of a moderate red heat by Dr. P. who from his greater facility in managing the fire from long habit, was thought most eligible to conduct the experiment. He repeatedly called the attention of the company to observe the stages of the process, and to remark that in every part of it that any voluntary deception on his part was impossible.

“In about a quarter of an hour, from the projection of the powder, and the placing the crucible in the fire, he observed to the Company, who on inspection found his observation true, that the mercury, though in a red hot crucible, showed no signs of evaporation, or even of boiling: the fire was then gradually raised, with attention on the part of the company, and repeated calls for that attention from Dr. P. that no undue addition might be made to the matter in the crucible; in a strong glowing red, or rather white-red, a small dip being taken on the point of a clean Iron Rod, and when cold, the *scoriae* so taken and knocked off, were shown to the company and found replete with small globules of a whitish coloured metal, which Dr. P. observed to them could not be Mercury as being evidently fixed in that strong heat;* but as he represented to them an intermediate substance between ☿ and a more perfect metal.

* Mercury vaporizes at about 662° F.

"A small quantity of Borax (brought by Mr. R.) was then injected by him and the fire raised, but with the same precautions on the part of Dr. P. to subject every thing to the minute inspection of the persons present; and after continuing it in a strong red-white heat for about a quarter of an hour, the crucible was carefully taken out, gradually cooled: on breaking it, a globule of yellow metal was found at bottom, and in the scoriæ smaller ones, which collected and placed in an accurate ballance by Mr. Russell, were found to weigh *fully* Ten Grains. This Metal was in the presence of the above mentioned Gentlemen sealed up in a phial, impressed with the Seal of Mr. Anderson, to be submitted to future examination, though every one present was persuaded that the metal was gold.

"This seal being broken the next morning, in the presence of the former company, and of Captain Austen,* and the metal hydrostatically examined, the weight of the larger globule (the others being too minute for this mode of examination) was found to be in air 9 Grains and a Quarter, and in distilled water of temp. Fahren. 50 plus, it lost, something more than $\frac{3}{8}$ (but not quite an half) of a grain: the difference was not appreciable, as no smaller weight than the eighth of a grain was at hand, but was judged by all the company to be nearly intermediate; i.e. $\frac{7}{16}$:—at half a grain the sp. gr. would be rather more than 18 : 1; if only $\frac{3}{8}$ were lost in water the sp. gr. would exceed 24 : 1. the intermediate would be 21 : $\frac{1}{7}$ nearly; but as the loss seemed rather more than the

* I have not been able to trace any particulars about this witness. In Banks' copy of Price's pamphlet (now in the British Museum) is a list in his handwriting of the witnesses who attended Price's experiments; but Austen's name does not appear in it.

intermediate, though apparently and decided (*sic*) less than half a grain, the specific gravity must have been nearly as 20 : 1. and in this estimate all present acquiesced.*

"After this hydrostatical examination, the globule was was (*sic*) flattened by percussion into a thin plate, and examined by Mr. Russell in the manner of artists for commercial purposes; on finishing his scrutiny he declared it to be as good gold as the grain gold of the *refiners*, and that he would readily purchase such gold as that which he had just examined at the highest price demanded for the purest gold.

"The plate being then divided, one half was before the company sealed up by Mr. A. to be submitted to a trial of its purity, which Dr. P. proposed, requesting his friend, Dr. Higgins, of Greek Street, to make; the remainder being put into Aq. Regia of Nit. acid and Sal. Ammon. afforded a solution sufficiently rich, before the company separated, to yield with sol. of Tin, a richly coloured crimson precipitate.

"Capt. G. was accidentally absent when the precipitate was made, but saw it next day. In about four hours the portion of metal employed was completely dissolved, and the next morning before Capt. and Mr. D. Grose, and Mr. Russell (Mr. A. being prevented from coming). The solution being divided into three portions the following experiments were made.

"To the first portion, diluted with water, was added a quantity of Caustic Vol. Alk. and the precipitate,

* It must be remembered that in 1782 chemical balances were very far from modern standards of accuracy. The method here indicated is the usual plan of weighing the body in air and in water, from which data the sp. gr. is easily calculated. The figure obtained (20 : 1) pointed unequivocally to the metal being gold.

which was copious, being duly separated and dried, about a grain of it,* placed on a tin plate, was heated and found to explode smartly; this experiment was repeated three times.

"To the second portion, diluted, was added a portion of Sol. of Tin, in Aq. Reg. A beautiful crimson coloured precipitate was immediately formed in considerable quantity: which when dried, was mixed with a soluble fritt, composed of flint-powder, and the fluxes proper for the Ruby Glass of Cassius, in the proportion of 5 grains of of (*sic*) the precipitate to 3ij [1 oz.] of the frit, and in a vitrifying heat afforded in about three hours a transparent glass, which by heating again, assumed an elegant crimson colour: and the remainder which continued in the fire also acquired a bright red colour.

"The third portion being mixed with vitriolic Ether, imparted to it the yellow colour given to this fluid by solutions of Gold: and the Ether being evaporated in a shallow vessel, a thin purplish pellicle adhered to the side, spotted in several places with yellow.

"Dr. Higgins soon after receiving the piece of Metal, favoured the Author with an answer, in which he notified that the packet came to him under the proper seal: That he was well satisfied of the *purity* of the gold he received; and that he considered the authors experiments as exclusively sufficient to have ascertained the nature and purity of the metal."

This extract brings out a number of points common to the whole series of experiments. One notes, first of

* The process formed the well-known "fulminating gold," mentioned by Pepys in his diary as early as 1663 (November 11th).

all, the eagerness shown by Price to ensure that his ingredients and their manipulation should be free from all suspicion of trickery. Probably, his eagerness defeated its own end; had he stressed the point less, less would afterwards have been made of it.

Secondly, it will be seen that the yield of gold was exceedingly small—although it was well outside the limits of experimental error. He obtained enough gold for a series of convincing tests; on the other hand, not enough to remove the suspicion that it might have been present beforehand as an accidental impurity. The most singular feature of the experiment, as described, is the fact that the mercury “showed no signs of evaporation, or even of boiling.”

The table on pages 216 and 217 gives a summary of all Price’s public experiments.

Needless to say, such reports of the demonstrations as found their way into print created an enormous sensation; and, not unnaturally, a good deal of adverse comment, duly noticed by Price when he published, in the autumn of 1782, his *Account of Some Experiments on Mercury . . .*” In his Introduction he remarks:

“Previous to this publication the Author has had frequent opportunities of hearing the opinions of many concerning its subject. Some say that they cannot account for the Theory of the process, and *therefore* that the fact is not true. Others ask, if it be true, is it profitable?*

Illiberal minds suggest that the whole was a trick, and without knowing or enquiring what evidence it rests on, modestly call the Author a knave and the Spectators fools:—And some Heroes

* It was certainly not profitable. Price is believed to have computed that the powder expended in one of the experiments, which had afforded about £4 worth of gold, had cost him some £17 to prepare.

of incredulity, declare that they would not believe it though they saw it with their own eyes and touched it with their own hands.”*

He asks how, by any deceit, he could keep Mercury from boiling in a red heat (Exp. II) or, when boiling, fix it almost instantly by adding not more than $\frac{1}{480}$ of its weight (Exp. III).

He *might* (but not in front of twelve or fourteen spectators) have conveyed metal into the crucibles, but in Exps. IV and V the silver was enriched with about 8 times as much gold as the powder projected on to it.

“. . . He may further ask (though this is not properly an argument with the public at large, but only with those who know his situation) what could induce him to take such laborious and indirect methods of acquiring sinister fame: possessed as he was of total independence, and of Chemical reputation.”

It will be noticed that on his title page he described himself as M.D. This was a complimentary degree, conferred on him by his university on July 2, 1782. It seems to have been generally assumed, then and afterwards, that this honour was paid him as the reputed discoverer of the Philosopher's Stone. He was at some pains to refute this in a second edition of his pamphlet, which appeared early in 1783:

“He also begs leave to remark, that the Gentlemen who in some of the public prints represented his late degree as confer'd in consequence of these experiments, must have been misinformed. There was not the least connection between them, as is well known

* Helmholtz, the great German physicist, was a super-Thomas of this kind. “I would not,” he remarks, “accept any abnormal phenomena on the mere testimony of my eyes.”

No.	Witnesses.	Materials.	Powder.	Result.	Date.
I	Rev. B. Anderson Capt. F. Grose Mr. Russell Ens. D. Grose	Hg ($\frac{1}{2}$ oz.) Borax Charcoal Nitre	Red ($\frac{1}{2}$ gr.)	Gold (10 grs.)	6.5.1782
II	Sir P. Clarke Dr. Spence Rev. B. Anderson Capt. Grose Mr. Russell Ens. D. Grose	Hg ($\frac{1}{2}$ oz.) Charcoal (1 oz.) Borax (2 dr.) Nitre (1 scrp.)	White (1 gr.)	White metal (13 grs.) Partial failure owing to using too much charcoal	8.5.1782
III	Rev. B. Anderson Capt. Grose Ens. Grose Mr. Russell	Hg ($\frac{1}{2}$ oz.) Charcoal Borax	White ($\frac{1}{2}$ gr.)	Repetition of No. II White metal (4 grs.)	9.5.1782
IV	As for III	Silver (60 grs. = 1 dr.) Flux as above, more borax added during fusion	Red ($\frac{1}{2}$ gr.)	Crucible cracked. Flux escaped, but no silver. Remaining metal found to contain gold	9.5.1782
V	As for IV + J. D. Garthwaite (who was also present at finale of Experiment IV).	Silver (30 grs.) Borax Charcoal Some "glass of borax" (to avoid moisture of crude borax) added later	Red ($\frac{1}{2}$ gr.)	Metal of original weight, but containing gold NOTE.—Silver of Experiment V (30 grs.) did not contain so much gold as that of IV (60 grs.), but the proportion was the same = $\frac{1}{3}$	9.5.1782

VI	Sir P. N. Clarke Rev. B. Anderson Capt. Grose Dr. Spence Ens. Grose Mr. Hallamby	Hg (2 oz.) Rubbed with a drop or two of Vitreous Ether	White (1 gr.) Rubbed into Hg for 3 minutes	Bead of silver (29 grs.), formed in Hg Experiment repeated 18.5.1782 before Manning, Rev. Dr. Fulham, Anderson, Robinson, and Spence. Result, 12 grs. silver in proportion of 28 : 1, as before	15.5.1782
		Hg (5 dr.) Rubbed with Vitre- ous Ether	Red ($\frac{1}{4}$ gr.)	Bead of $6\frac{1}{4}$ grs. gold obtained	
		Hg (3ij, about)	Red ($\frac{1}{8}$ gr., about)	Rather more than 1 gr. of metal, containing gold	18.5.1782
VII	Lord Onslow Lord King Lord Palmerston Sir R. Barker Sir P. N. Clarke Rev. O. Manning Rev. B. Anderson Rev. G. Pollen Rev. J. Robinson Dr. Spence W. M. Godschall W. Godschall W. Smith Mr. Gregory Mr. Russell	Hg (3ij) (rubbed up with a few drops of Vitreous Ether)	White (1 gr.)	10 gr. silver (about $\frac{1}{4}$ of total mass)	25.5.1782
		Hg ($\frac{1}{2}$ oz.) Charcoal Borax	Red ($\frac{1}{2}$ gr.)	About 10 grs. gold	
		Hg (30 oz.) Hg (1 oz.)	White (12 grs.) Red (2 grs.)	> 600 grs. "Fixed white metal" (Silver) "Fixed and tinged metal" (Gold)	28.5.1782 (Before <i>some</i> of the previous company)

to almost every member of the Convocation; nor, indeed, could there be, since the degree was given some time *before* the experiments were known in *Oxford*. It was conferred expressly on account of his *former* chemical labours. . . .”*

Oxford may be a mental backwater;† but it is a little difficult to believe that no one there had heard, by the end of June, of the experiments, made by an Oxford M.A., which had been the principal topic of conversation in London for some six weeks past. And, indeed, when the question was raised (some years after Price’s death) in *The Gentleman’s Magazine*, a member of Convocation, in defending the grant of the degree (which, he admits, was strongly opposed), could find no better reasons to justify it than that Price was a gentleman commoner, that he had behaved with sobriety, and that “he was going abroad, where the degree would be a recommendation to him.”‡ He added, it is true, that he was reputed to be “the best chemist in the kingdom,” but it is difficult to understand how Convocation could test this assertion, since Price had then published nothing whatever. In any case, it seems not improbable that, but for the interest attaching to his alchemical experiments, Price might have whistled for his “M.D.Oxon.”

* In the original, the text is in italics, and the words here italicized in ordinary type.

† “It is scarcely necessary to say that, in this hot competition of bigots and slaves, the University of Oxford had the unquestioned pre-eminence. The glory of being farther behind the age than any other portion of the British people, is one which that learned body acquired early, and has never lost” (Macaulay, *Sir James Mackintosh*).

‡ *Gentleman’s Magazine*, 1791, p. 893, in a letter signed “R.C.” His antagonist, “L.L.,” in a rejoinder (p. 1009) pointed out that none of the reasons adduced could be taken seriously, and that the third, in particular, reminded him of the conscientious gun-makers, “who lay apart all barrels so faulty as to be likely to burst when once heated, that they may not be used in home consumption, but all exported together to furnish the African Negroes with musquetry. . . .”

By the autumn of 1782, he had reached the peak of his career. His pamphlet, for all its cautious title and sober style, had brought him acclamation and celebrity; "monarch of all he surveyed" at Guildford, he was also a social success in London. Majesty itself condescended, no doubt with turkey-like gobblings and much vain repetition, to inspect and approve specimens of his artificial gold and silver.* A baronetcy, at least, could scarcely be avoided; and who could say what further honours were in store?

Yet there is much ground for believing that Price knew, even before he sent his pamphlet to press, that his apparent success was purely ephemeral—a house of cards which a breath of adverse criticism would destroy. No amount of testimony could get over the facts that his experiments had not been made under strict test conditions; that he had not divulged the composition of his powders; and that he was not prepared either to submit them to analysis or to repeat the experiments. He was compelled, we must imagine, to adopt this attitude—and he struggled in vain to explain it away. Here are his own words:†

"... The whole of the materials producing the extraordinary change in the metal employed, was expended in performing the processes which are now to be related: nor can the Author furnish himself with a second portion, but by a process equally tedious

* "... These last portions of Gold and Silver . . . have had the honour of being submitted to the inspection of His Majesty; who was pleased to express his royal approbation. This honour may be mentioned with the less impropriety, as it is conferred by a Sovereign equally revered for his patronage of Science and beloved for his amiable condescension."

One is reminded of the courtly French chemist who, giving a demonstration before Louis XVI, began: "I have here certain volumes of hydrogen and oxygen, gases which will shortly have the honour of combining before Your Majesty."

† *Loc. cit.*, p. v.

and operose, whose effects he has recently experienced to be injurious to his health, and of which he must therefore avoid the repetition.—The repetition, indeed, would avail but little to establish the facts, or gain belief. That more would believe if more had been present, is indeed true; but as the Spectators of a fact must always be less numerous than those who heard it related, the majority must at last believe, if they believe at all, on the credit of attestation. . . .”

In other words, he invited the public to regard and tolerate him as a dog in the manger, possessed of a most valuable bone which he would neither share nor eat. If he had gone out of his way to devise a plan which would unite all shades of opinion in condemning and vilifying him, he could scarcely have acted differently. Peasants and politicians alike regarded him as a man who could make the whole kingdom, impoverished by the loss of its American possessions, fabulously rich and powerful in the twinkling of an eye—and who would not; scientific men were shocked by his lack of candour and his unscientific secrecy; men of rank began to regard him as an adventurer; his personal friends felt their loyalty severely and unfairly tested. Kirwan and Higgins, for example—both chemists of standing, one of whom had signed his R.S. candidature certificate, while the other had analysed some of the artificial gold—both pleaded earnestly with him, for the sake of his reputation, either to admit that he had been deceived by some flaw in his operations, or to put his cards on the table and disclose the formula and manufacture of his powders. But he could not bring himself to do either.

Indeed, he never seems to have formed a clear judgment on this crucial point. At one time, he appears to

have half admitted that he had deceived himself. Thus Priestley, writing to Wedgwood, remarks:*

(*Birmingham, Oct. 10, 1782.*)

" . . . You have heard of a pretended transmutation of quicksilver into gold by Dr. Price. Yesterday I had a letter from Mr. Kirwan, who, after some account of it, adds: 'But I have lately seen him, and he has owned, that he believes that he was deceived, and that his mercury previously contained gold: that he bought it from the makers of *Or moulu*,† &c. I said so much to him, that he is now satisfied to pass only for a mere able extractor of gold, and says he uses a preparation of arsenic, of which I persuaded him to promise he would give a paper to the Royal Society. If you have not heard this before you will like to have the information. . . ."

But although such may, for a time, have been Price's conviction, he speedily altered it. To the 1783 edition of his pamphlet (now entitled only "An Account of Some Experiments on Mercury"—"Silver" and "Gold" being omitted)—he prefixed an "Advertisement," in which he says:

" . . . The Author avails himself of this opportunity to observe, that the reports circulated respecting a mistake in these processes from employing Mercury accidentally impregnated with Gold by having been used in the manufacture of *Ormolu*, &c., are entirely without foundation; as indeed is evident from Exp. IV and V, and Exp. VII, p. 26. 1 5 (1st Edit.).‡

* *Scientific Correspondence*, New York, privately printed, 1892, p. 42.

† *Ormolu*—an alloy of copper and zinc, whose colour, closely approximating to that of gold, was generally enhanced by a wash of gold lacquer. In Price's time it was much used for furniture mounts and other pieces of cabinet-work.

‡ The passage referred to is: "half an ounce of Mercury revived from Cinnabar, brought by the Rev. Mr. Anderson, was by him placed in a small

“He has not on enquiry found any reason to believe that the Mercury employed in these Experiments had in it more precious Metal than is usually contained in all Mercury:* and, indeed, the notion of manufacturers suffering Mercury so richly impregnated to pass out of their hands, without extracting all the Gold separable by the *common* methods, is so very improbable as scarcely to require refutation.”

Two other passages from the same “Advertisement” (which may be regarded as a final manifesto, flung out by Price as a gesture of defiance in the face of an ever-growing body of adverse opinion) are important enough to deserve quotation:

“The Author of the following account intends to publish an appendix, in which he will attempt to explain the principles of some of his processes, and to shew their analogy to experiments related by Chemists of reputation:—as the collecting these must require some reading and attention, he cannot assign precisely the time at which the appendix will appear, but it will be prepared with all convenient expedition. . . .

“Many other remarks which he has heard might be removed by an attentive perusal of the narrative. He has only to add, that he is sorry his account, to which he himself gave only the unassuming title of ‘Experiments on Mercury,’ should have been held out to the World as announcing the discovery of the

round English crucible. . . .” In Experiments IV and V, grain silver, bought from a refiner, was used—not mercury. (Original text is in italics.)

* In view of this statement, a footnote added to this edition is decidedly ambiguous: “The Author, by the words *product*, *produced*, and the like, here and in other places, means only to express that a quantity of precious metal was really obtained; and neither to affirm or deny any speculative opinions relative to the action of the Matter projected on the Mercury, or concerning the *manner* in which the precious metal is contained in Mercury.”

Philosopher's Stone, which in the *usual* sense of the word, he perhaps as well as others, thinks merely chimerical."

It seems likely that this was written towards the end of 1782, when Price was still trying to temporize with the world—and with himself. But the toils were closing around him. What disclosures the appendix* was designed to contain—whether it was intended to be an honest account of the preparation of his powders or only a chain of theoretical reasoning (fortified by quotations from the early alchemists, backed up by Van Helmont, Helvetius, and the like) which should supply, if possible, an avenue of retreat from a position which had become untenable—will never be known. It never appeared, and no MS. of it has been found.

The relations subsisting at this period between Price and the Royal Society are involved in a good deal of obscurity. Officially, the Society took no cognizance of his experiments or of his pamphlet. Under its rules, it could scarcely do so. No MS. account of the alleged transmutations had been sent in by Price; and although he may have sent a complimentary copy of his pamphlet to the R.S. Library,† this could not have been regarded as being on the same footing as a personal communication of his results designed for the official consideration of the Society's Council. Moreover, so long as Price clung to his "secret," and refused to disclose the composition of his powders, the Royal Society—which has never exhibited the slightest toleration of obscurantism—would most undoubtedly have refused to give the slightest attention to his results.

* Price announced that it would also embody his abstract of Boyle's narrative (omitted from the second edition of his pamphlet) and "some references to other writers."

† No trace of such a present is to be found in the Society's records.

But, although definite proof is lacking, there can hardly be any doubt that it was widely felt—and by none more strongly than by Sir Joseph Banks, P.R.S.—that Price's doings were not such as were calculated to enhance the prestige of the Society as a body, or to increase the estimation in which its Fellows were held by the general public. "Here (we can imagine Banks growling to himself) is a youngster with more money than sense, pitchforked into our body on the understanding that he is going to do valuable work in experimental chemistry on sound lines. No sooner does he write F.R.S. after his name than he comes out with a fantastic story about the essential truth of alchemy, backed up by a few experiments made before credulous bumpkins and town rakes—experiments in which he says that he used a secret powder which he can't or won't make again, and whose composition he won't disclose. He may have bubbled the King (God bless Him for a kindly fool), but he won't bubble Joseph Banks. I believe the whole story is a pack of lies, and damme, but I'll find out before I'm much older. He shall repeat his damned experiments before a committee of my nomination, or show me cause why I shouldn't lay his name before the Council, with a request that they will take into their consideration whether his conduct has been such as we have a right to expect from our members."*

* The outlines of Banks' character are pretty well known; and while the above outburst is, of course, purely imaginary, I think that it may be regarded as giving a fair picture of what Banks thought about Price and his doings. He was undoubtedly very much of an autocrat; and while he generally played the part of a benevolent despot (for which his presence and ample fortune well fitted him) those who had the misfortune to cross him usually emerged from the conflict with a confused impression that they had become involved with the business end of a mule. It may be added that he was a sciolist rather than a scientist, and that natural history was the only branch of science about which he knew more than the average squire.

Let us say, at any rate, that Banks, in a Chucks-like manner, put pressure on Price, "in the most delicate manner in the world," to repeat his experiments, or take the consequences. And the consequences—expulsion from the Royal Society and from "Society," general infamy, the revocation of University honours and degrees, the open derision of the "public prints," and the public whose trough such papers fittingly supplied, the quiet scorn and sarcasm of some erstwhile friends, and the scarcely more bearable pity of others, the shame and the disgrace—must have presented a terrifying vista to the sensitive mind of a young man who had not lived long enough to discover that there are very few things in life that really matter, and that public applause and public blame are not among them.

Apart from any question of mere social infamy, there is some reason to think that, as matters stood, his life itself was not safe. The end of the eighteenth century was an era of considerable "violence to the person"; and it requires little imagination to see Price's house sacked and himself pursued by a raging mob (as happened to Priestley a few years later): or to visualize him being kidnapped by some precursor of Carl Peterson,* and tortured to extract his secret.

In January 1783 he left London and went down to Guildford, having (it appears) given an undertaking to his friends that he would use his utmost endeavours to prepare more of his powders and repeat his experiments. Previously, he had tried (and failed) to obtain information about some German alchemical recipes which, he thought, might prove more serviceable than his own.

* The reference is to the series of "Bulldog Drummond" novels published by "Sapper." He has seen fit, alas, to terminate Petersen's existence in a manner which, unlike the demonstration of *ju-jitsu* given by Sherlock Holmes above the Reichenbach Fall, leaves us no hope of a resurrection.

It is significant of his state of mind that, on his arrival, his first act seems to have been to draw up his will, beginning with the phrase "Believing that I am on the point of departing from this world. . . ."

What, exactly, was his state of mind? Apart from a conviction of imminent ruin (which, on almost any hypothesis, it is safe to assume had taken possession of it) nothing can certainly be deduced as to this, but it may be of interest to examine the alternatives.

In May 1782 Price performed, before selected witnesses, experiments which, *prima facie*, tended to show that he had actually performed the transmutation of mercury into silver and into gold. We have, as Mr. Gladstone was so fond of remarking, three courses open to us. We may conclude:

- (a) That he was a deliberate impostor.
- (b) That he deceived himself, and in so doing also deceived others.
- (c) That he actually accomplished transmutation.

There would be nothing remarkable in his being a deliberate impostor. Most of the mediæval alchemists were cut to that pattern: and, indeed, there is something in the very name of gold which infallibly brings out the mean and base side of human nature; just as association with that noble animal the horse tends to develop a human type—bookies, tipsters, tic-tac men, gangsters, and the like—of which the world would be well rid. It is true that he was of independent fortune, but there are many instances of men having stooped to do for fame, or pride, or enmity, what nothing on earth would have induced them to do for money.

To reasoning like this, Price's defenders—if, indeed, at this distance of time, any such hardy persons can be

found—have no ready answer. Admittedly, if we assume that he was an impostor we have an immediate explanation of the marvellous element in the story: the pretended transmutations, the unvaporizable mercury, and so on.

But it is also to be noted that, by accepting a theory which gets over the main difficulties, we elevate the more reasonable parts of the story into mysteries of their own. If Price were honestly minded, it would be natural that, at all hazards to his fortune and peace of mind, he should strive to convince others of the reality of his discoveries. But, in the name of common sense, why should a man build a stone wall for the purpose of running his head against it? Why should he deliberately engage in a campaign of deception which, it was obvious, could not be long sustained, and in which he must ultimately perish? Why should he choose to go up like a sky-rocket, if he knew that he must come down like the stick? If he were unprincipled enough, and adroit enough, to make a good trickster, how could he have been, at the same time, fool enough to undertake anything of the kind? No sane man, one feels, could have been, at the same time, so much of a knave and so much of an ass.

It is, of course, tempting to suppose that Price was a monomaniac: that he had become so as the result of a long course of alchemical reading and experiment: and that, as in the parallel case of the modern "snow-bird,"* his obsession lent him cunning and plausibility enough to deceive all those around him, while it so blunted his moral sense that he saw no harm in what he was doing. Or we may more charitably suppose that his *idée fixe* had so subjugated his reasoning powers that

* Female cocaine-addict.

—like the earnest believers in spiritualism whose faith in some particular “medium” survives the most crushing possible exposure in their own presence*—he drew unwarrantable deductions from his own data, and unconsciously selected only those points favouring his views, discarding the remainder. On the other hand, after a careful perusal of his pamphlet I confess to a strong impression that he was of sound mind when he wrote it; and therefore, presumably so when he made his experiments.

As to the possibility of his having actually effected the transmutations which he claimed, it need only be remarked at this stage that, unless the statements of Van Helmont, Helvetius, Boyle, and the considerable body of similar extant testimony be rejected on *a priori* grounds, it would seem that this can scarcely be ruled out. On the other hand, the conditions under which his experiments were made were certainly not stringent enough to exclude the possibility of fraud (deliberate or unconscious) and the most charitable verdict which can be passed on them is “not proven.” Once Price’s honour and veracity are questioned—and the question cannot be avoided—one looks in vain for corroborative testimony. His pamphlet remains the sole record of his doings. It is true that he informs us:

“ . . . The following succinct account [of the experiments] was read over to the respective witnesses of each experiment. He [the author] now requests publicly, as before privately, their confirmation, without the slightest fear of contradiction or dissent.”

But, so far as I can trace, no particle† of any such

* Although scarcely credible, this has repeatedly happened.

† Not even, to quote Lord Oxford’s celebrated peroration at Cambridge, “one jit or one tottle.”

confirmation was ever given by any one of the men to whom he so confidently and pathetically appeals—men who had once been proud to call him their friend.

Price spent the last months of his life at Guildford, in ever-growing seclusion and despair. He is believed to have attempted to prepare more of his powders, and to have failed. It may be that, like Dr. Jekyll,* he came to believe that his former success had been due to accidental impurities in his materials; or that he turned again to his former theory that his mercury had not been free from gold at the outset: or that he had simply lost whatever faith he had ever possessed in his own operations. What is certain is that he gradually was forced by circumstances into that black pit of despair whose edge most men have skirted at some period of their lives, and whose depths have seemed, to only too many tortured minds, the one way out of their miseries. The game—if it were a game—was up. The tragedy had reached its last act. His friends forsook him one by one; he dared not show his face in London and, indeed, hardly ever left his house.

Yet in the last days of July 1783 he seems to have shaken off his apathy, and to have made his way to London for the purpose of formally inviting the Royal Society, or such members of it as could conveniently attend, to witness a repetition of his experiments at Guildford on August 3rd. Three only attended. Price

* "In the year 18—, Dr. J. purchased a somewhat large quantity from Messrs. M. He now begs them to search with the most sedulous care, and should any of the same quality be left, to forward it to him at once. Expense is no consideration. The importance of this to Dr. J. can hardly be exaggerated."

So far the letter had run composedly enough; but here, with a sudden splutter of the pen, the writer's emotion had broken loose. "For God's sake," he had added, "find me some of the old!"—*The Strange Case of Dr. Jekyll and Mr. Hyde*, ch. viii.

received them cordially, but his face showed how much he felt the silent stigma of receiving this tiny and sceptical band of inquisitors in the place which, a few months before, had been full to overflowing with a fashionable and admiring crowd. He led the way to his laboratory, and invited an examination of his apparatus. Before this had ended, he was lying on the floor insensible, having poisoned himself with prussic acid. A doctor was called in, but the case was hopeless from the first.

*Cut is the branch that might have grown full straight,
And burnéd is Apollo's laurel-bough,
That sometime grew within this learned man.
Faustus is gone. . . .*

According to the only report of the inquest which I can trace,* the verdict was "Lunacy, upon general evidence of his having, in many instances, acted like a man insane." "It was 'generally supposed,' in the neighbourhood of Guildford, 'that his studies hurt his mind.' . . . His feelings also were nice, and he could not stand the ridicule of the news-papers about the gold business. . . . This, and other things together, 'overset him.'"

His epitaph still stands, and lies, in Stoke Church, to the following effect:

"Near this place are deposited the remains of James Price, M.D., F.R.S., . . . who departed this life the 31st of July, 1783, aged 25 years. *Heu! qualis erat.*"

There is little doubt that the true date is August 3,† 1783, and none at all that he was thirty-one.

* A letter written by one of Price's neighbours eight days after the event—quoted in *The Gentleman's Magazine*, 1791, p. 894.

† It was certainly either the 3rd or the 8th of August. He was buried on the 9th.

Semler.

Price's youth, wealth, and tragic end have combined to make him a romantic figure, and to win for him the title of "The Last of the Alchemists";* but the term is a complete misnomer. For example, a German rival and contemporary of Price attracted, for some time, a good deal of attention, and added to the gaiety of nations in no small degree.

Professor Semler, of the University of Halle, had made theology his life-study; but in his early days he had indulged a passion for alchemical research. He was of an engagingly simple turn of mind, and an astute Jew took advantage of this to fool him to the top of his bent by professing to be possessed of various wonderful alchemical secrets which he had picked up in Barbary. Disheartened, Semler abandoned his laboratory, and betook himself to the study of the Mishna.

However, *on revient toujours à ses premiers amours*, and in his later years he began again to dabble in chemistry at a time when one Baron Hirschen happened to be advocating the transcendent virtues of a patent medicine which he called the "Salt of Life," which seems to have been a lineal descendant of Berkeley's tar-water, and an ancestor of many a like nostrum of our own day.

Semler took a good deal of "Salt of Life" and, having survived this successfully, deduced therefrom that it must have done him a great deal of good; an opinion to which he devoted several treatises, no more readable than such

* There is an unconsciously funny article of this title in *All the Year Round*, June 13, 1863. The writer makes Price perform his experiments in 1787 at Salisbury, of which he is Dean! A mysterious and hostile stranger proves to be the Secretary of the Royal Society, and his return to London is followed by an imperative summons from Banks, citing Price to repeat his operations before that body. He complies; and poisons himself, after an appeal for clemency, in the ante-room!

matters generally are. He was then struck with the idea that if Hirschen's nostrum were a universal medicine it might be able to cure the base metals as well as the human frame, and to convert them into gold. He made a solution of the "Salt of Life," and put it in an earthenware jar near a stove.

In a few days he found that the solution had precipitated some thin flakes of metal, which proved to be gold. He repeated the experiment several times, always with the same surprising result; finally, he published an account of it—greatly to the delight of Hirschen, for in a short time it became *de rigueur* throughout Germany to ornament the fireplace with a jar of dissolved "Salt of Life." These, however, remained purely ornamental, not at all to the satisfaction of their owners.

Worse was to come. An eminent German chemist, one Klaproth, analysed the "Salt of Life," and, caring nothing (like the B.M.A.)* for its proprietor's fulminations, published the result; a mixture of Glauber's salt† and sulphate of magnesia, which, in the nature of things, could not contain the slightest particle of gold.

The known probity of Semler's character made it obvious that he must have been deceived in some way; but he did not accept this view, and a prolonged paper warfare followed, in which the chemists insinuated that Semler, as a theologian, was bound to be a fool, while the theologians retorted that Klaproth, as a chemist, was not improbably a knave. Finally, Klaproth agreed that if Semler would send him some of his gold-bearing solution, he would make a public analysis of it.

The result showed the presence of something which

* In their *Secret Remedies*, London, 1909, and its sequel.

† Decahydrated sodium sulphate, $\text{Na}_2\text{SO}_4 \cdot 10\text{H}_2\text{O}$.

looked like gold, but proved to be "Dutch metal," a kind of brass.* A legal inquiry was ordered, and the solution soon appeared. Semler had an old man-servant, much attached to him, who had thought it a pity that, if his master wanted to find gold in the solution, this should not somehow occur. Accordingly, he used to buy small quantities of gold-leaf, and surreptitiously introduce them into the jar at intervals. Being compelled, however, to leave home to draw his pension, he instructed his wife to carry on the good work, giving her money for the purpose. The good lady, having little sympathy with the Professor's experiments, and a better use for the windfall, spent most of it at the local tavern on "Dutch courage," and used the balance to purchase "Dutch metal."

Theodore Tiffereau.

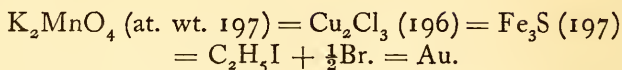
There existed in France at the end of the nineteenth century (and, for all I know, still exists) a body terming itself "L'Association Alchimiste de France."† It was founded by Albert Poisson (d'Avril?), who died in 1894. It boasted a Secretary-General and seven Councillors, who met annually. In July 1897 it also had two Honorary Members, Camille Flammarion and (of all people) August Strindberg!‡ The latter, although better known as dramatist, novelist, and woman-hater, seems at least to have dabbled in alchemy of a rather futile kind. Here, for example, are some "correspondences" which he claimed to have discovered and which, to those who like that sort of thing and can shut their eyes to arith-

* Also called "tombac."

† My account of the French alchemists is based, in part, on H. C. Bolton's very entertaining *Modern Alchemy*, New York, 1897.

‡ Dr. S. H. Emmens was subsequently added to this list.

metrical discrepancies, are no doubt full, like Gilbert's "Basingstoke,"* of hidden meaning.



It will be noticed that he uses 196 and 197 indifferently as the atomic weight of gold.† Incidentally, Fe_3S is a hypothetical compound, whose atomic weight would be 200.

And here, from the same source, is a nineteenth-century alchemical recipe:

"Put into a crucible layers of sheet tin and of powdered vitriol; place over it another crucible pierced with a hole for respiration. Heat in an intense fire. But a flux must be added to the crucible to prevent melting, viz. one kilo litharge, one kilo clean white sand—mix and add to the crucible at a red heat. Remove the yellow oil with an iron spoon, and put it aside. The two compounds have not lost weight. This oil is dry water, a fire, a salamander. . . .

"You obtain a metal of a golden yellow, having a density of 241, not capable of being minted. This is changed into ordinary gold."

The zoological aside, occurring at an important point in the process, is rather confusing. I understand that those who have attempted to make gold in this manner have concluded that some important step has been omitted from the recipe, or that the printer was not feeling quite at his best when he set it up.

But if the names of Flammarion and Strindberg lent a certain amount of éclat to "L'Association Alchimiste de France," the doyen of French alchemists in modern

* See (or, better, hear) *Ruddigore*.

† It is actually 197.2.

times is certainly Theodore Tiffereau; who, after a long life of obscurity, attained a fleeting notoriety in 1899. No sooner had the *New York Herald* published its account of how Dr. Emmens was making gold—and not only making it, but selling it to the U.S. Mint (a perfectly correct statement, by the way)—than the French Press, after its manner, proclaimed *urbi et orbi* that Emmens was a base imitator of the illustrious French genius, Theodore Tiffereau!

Tiffereau was then an old man. His work, such as it was, had been done many years before. In 1854–55 he bombarded the Académie des Sciences with a series of six memoirs on transmutation, describing a process which he claimed to have discovered in 1849 at Guadalajara, Mexico, where he was earning a precarious existence as a photographer. Here is an extract from his third memoir, presented May 8, 1854:

“My first success was obtained at Guadalajara. The circumstances were as follows:

“After having for two days exposed pure nitric acid to the action of the solar rays, I placed in it some filings of pure silver alloyed with pure copper in the same proportions as those of the alloy used for making silver coins. A brisk reaction was manifested, accompanied by a very copious disengagement of nitrous gas, after which the liquid, on becoming quiet, allowed me to see an abundant deposit of the filings quite intact and agglomerated in a mass.

“The disengagement of nitrous gas still continuing, I left the liquid to itself for twelve days, and I remarked that the aggregated deposit became sensibly augmented in volume. I then added a little water to the solution without any precipitate being produced,

and I again allowed the liquid to rest for five days. During the whole of the time there was no cessation of the vapours.

"At the end of the five days I heated the liquid to ebullition, and maintained it at that temperature until no more nitrous fumes were given off, after which I evaporated the solution to dryness.

"The substance thus obtained was dry, dull, and of a blackish-green colour; it presented no appearance of crystallization; *no saline matter was deposited.*

"This residue was then heated with pure boiling nitric acid for ten hours, and I noticed that it became of a clear green colour without any change as regards its aggregation in small masses. I added a further quantity of pure concentrated acid; I again boiled the liquid, and then I at length saw the substance become aggregated and take on the brilliancy of natural gold.

"I collected this product, and I consumed most of it in a series of tests as compared with pure natural gold, but I was unable to perceive even the least difference between natural gold and the gold I had succeeded in obtaining."

According to Emmens, who examined a specimen of this Guadalajara gold, it was a little redder than ordinary gold, and consisted of a number of irregular fragments showing what looked like file marks. He did not consider that it could be "parted" gold, obtained by merely dissolving an alloy of silver, copper and gold in nitric acid.

Tiffereau's process was tested at the Paris Mint by M. Levöl, the chief assayer, "with little success." The result was communicated to the Académie on October 16, 1854. According to Emmens, M. Levöl, who

had furnished the silver for the experiment, and certified it as free from gold, became annoyed because in the course of the experiments small traces of gold kept making their appearance. He declared that the silver was obviously impure, and suspended the demonstration *sine die*.

Unfortunately, Tiffereau subsequently published a work in which he exposed his lack of chemical knowledge rather cruelly.* And in 1896 he sent another memoir to the Académie, in which he attempted to show that aluminium is a compound. His method of doing so was to leave aluminium and nitric acid in a sealed test-tube for two months. On opening it, he detected a smell "rather like" ether, and some crystals which "looked" like acetic acid. These being both carbon compounds, he concluded, somewhat hastily, that aluminium must be one also. Such are the mental aberrations of the aged.

Dr. S. H. Emmens.

Tiffereau's claims can be dismissed without much difficulty as the product of defective knowledge crossed with imperfect technique. A similar case occurred, during his last years, in America. One Edward C. Brice, of Chicago, applied in 1896 for a patent on a process of making gold from antimony. The patent was refused, whereupon Brice demanded, as was his legal right (I believe), that the process should be officially tested at the United States Mint. Three assayers accordingly conducted a conclusive test under Brice's eyes and in accordance with his instructions. All that it showed

* *Les Metaux sont des Corps Composés*, Paris, 1855. (New edition, Lermine, Paris, 1889). In this, Tiffereau enunciated his theory that transmutation is effected by the action of "the microbe of gold."

was, that by Brice's method it was possible to recover part of the (very slight) trace of gold which is present in all commercial antimony. Even for this purpose, the process was comparatively inefficient. It may be added that its discoverer's record was not adapted to bear searching investigation.

But hard on the heels of "the great Brice joke" came a most singular event. It gradually became known that Dr. S. H. Emmens, of New York, claimed to have discovered a substance, intermediate between gold and silver, which he had named "argentaureum," and which was capable of being changed into gold. Not only so, but he was actively engaged in manufacturing gold from silver in this manner, and selling the product in moderate quantities to the United States Mint. The matter first attracted universal attention in 1899, when the *New York Herald* came out with a "scoop" article, containing such headings as

THIS MAN MAKES GOLD AND SELLS IT TO THE
UNITED STATES MINT

IS DR. EMMENS A MODERN ROSICRUCIAN?

UNCLE SAM HAS BEEN BUYING HIS BRICKS FOR
TWO YEARS

As the last of these indicates, the *New York Herald* was a little behind the times. It had not heard of Emmens before; but others had, both in connection with chemistry and in many other lines of research. He was the author of many books on a great variety of topics: a work on logic, another on explosives, several novels, and many papers on chemistry, electricity, and metallurgy—even a book of poems. English by birth, he was a member of

several learned societies* and of the United States Board of Ordnance, and had invented a high-explosive, "Emmensite," which had been officially adopted by the United States Government. With regard to his alchemical doings, some account of these had been appearing at intervals in the United States Press since 1897.† It may be added that he published an outline of his work and methods, together with his correspondence on the subject with Sir William Crookes, in a book issued in 1899,‡ and that the following account is chiefly based upon this.

It appears to be a pamphlet written for sale at the Greater Britain Exhibition, at which Emmens was invited to exhibit his process of making "argentaureum"—which he did. It is ably and temperately written. If we are to regard Emmens as a crank, he is a very favourable specimen of the breed; and, after all, cranks keep the wheels turning.

He is convinced of the truth of the old theory that all forms of matter are ultimately one. He defends his "argentaureum" theory as follows:

"We find gold wherever we find matter (e.g. greenstone) that has made its way from the interior of the earth to regions within our reach, under conditions that have admitted of very slow cooling. We do not find it in ordinary lava streams where thermal energy has been rapidly dissipated. Yet lava and greenstone are composed of similar material from a terrestrial point of view; whence it becomes reasonable to infer

* Am. Inst. of Mining Engineers, Am. Chemical Society, Soc. Intern. des Electricians, U.S. Naval Inst., U.S. Military Service Inst. (and sometime Fellow of the Inst. of Actuaries, Gt. Brit. & Ireland).

† An article entitled "Changing Silver into Gold," by Herbert C. Fyfe, and based on information communicated by Emmens, appeared in *Pearson's Magazine* for March 1898.

‡ *Argentaureana, or some Contributions to the History of Science*, Stephen H. Emmens, Bristol, 1899.

that a non-auriferous limestone, subjected to the same natural laboratory treatment as an auriferous greenstone, is capable of producing gold by the transmutation of some of its own constituent particles.

"Also when, as a matter of fact, we find that natural gold *in situ* is invariably associated with silver, we cannot avoid the conclusion that such association is not portentous (*sic*).^{*} We are forced to admit that some kinship, as it were, exists between gold and silver—e.g. that in the course of natural chemical evolution silver becomes transmuted into gold, or gold into silver, or that some third substance exists which changes partly into gold and partly into silver."

Emmens claimed to have produced this third substance in his laboratory, by a method which he kept secret—but which apparently consisted largely of the mechanical treatment of silver by hammering it incessantly under conditions which allowed of the heat generated by the blows being rapidly conducted away. By his own account, the process was composed of five principal stages:

- (a) Mechanical treatment.
- (b) Fluxing and granulation.
- (c) Mechanical treatment.
- (d) Treatment with modified nitric acid.
- (e) Refining.

"I regard," he remarks, "the mechanical treatment as the *causa causans*. The fluxing and granulation serve, I think, merely to render the molecular aggregates susceptible of displacement and rearrangement." In producing his "argentaureum gold," Emmens used as his material Mexican silver dollars, certified by the

^{*} I imagine that the "not" is redundant, or else that "portentous" is a misprint for "fortuitous."

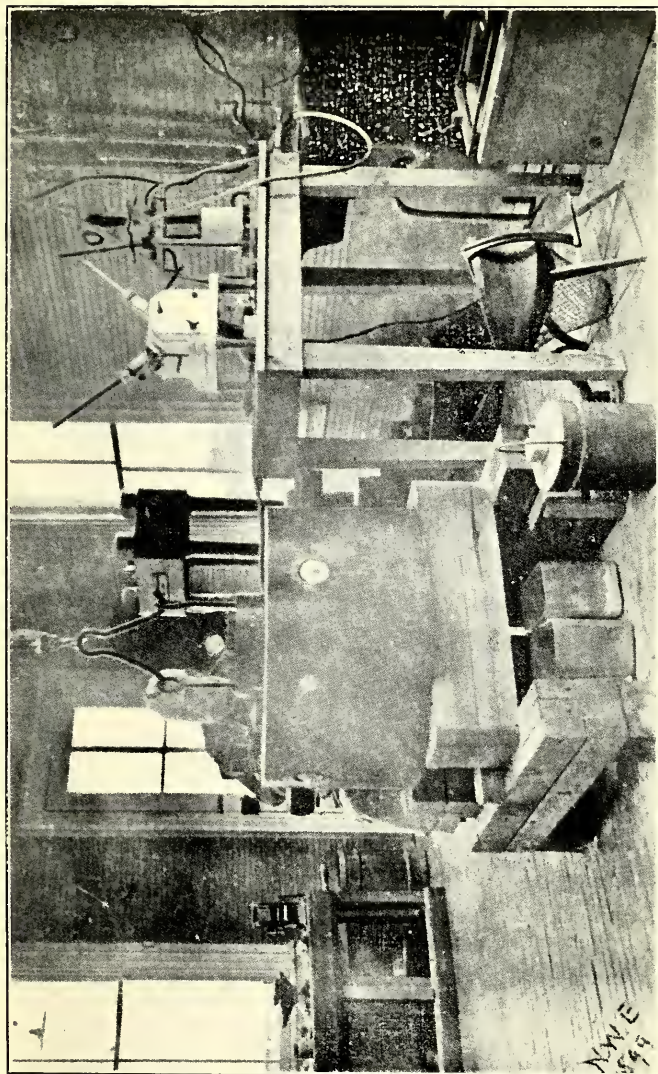


PLATE V

DR. S. H. EMMENS IN HIS LABORATORY

From "Argentaurand"

[Facing page 240]

United States Mint as containing "less than a trace" (i.e. less than one part in ten thousand) of gold. These he proceeded to change by his process, the mechanical treatment being applied by means of a "force-engine"—which seems, so far as one can follow the vague hints which he gives of its powers, to have been a cross between a pneumatic riveter and a hydraulic press.* The product, consisting of ingots containing an alloy of silver and gold in varying proportions (with occasional traces of other metals) he disposed of, after an official assay, to the United States Mint.

Here are the official figures for the amounts of "argentaureum gold" purchased by the United States Assay Office between April and December 1897.

Month.				Weight in oz.	Net value (£ sterling).
April	16.65	43
May	23.05	64
June	16.60	40
July	31.10	88
August	71.64	190
September	79.42	215
October	194.75	379
November	164.14	329
December (half month)	63.66	180

The results of the process were not very consistent, for the ingots sold in 1897 varied in fineness of gold from 305/1000 to 751/1000, and those sold in 1898 from 313/1000 to 997/1000—this last, of course, being practically pure gold.

The boom given to the subject by the *New York*

* See Plate V.

Herald did not last long. The proprietor of that paper, James Gordon Bennett, printed a public challenge to Emmens to give a demonstration of his process before a committee. This was at once accepted. There only remained the question of paying the necessary expenses and selecting the witnesses. Both proved insoluble problems.

Emmens estimated that it would cost about £2,000 to equip a new laboratory for the demonstration; on the other hand, if this were given in his own, the cost might amount to £3,000, since if the committee suspected fraud they could not satisfy themselves on the point without taking up the floors, and so on. On the face of it, this seems reasonable; but Gordon Bennett began to think that the gold bricks in question were of a kind with which the public was already familiar.

It was next found that it would be no easy matter to get any scientist who valued his reputation to sit on the proposed committee. Mason (of the United States Assay Office) and Nikola Tesla, for example, flatly refused, and many others followed their example—one which, on the face of things, showed commendable prudence. They could scarcely gain reputation by taking part in such a proceeding; on the other hand, they were by no means unlikely to lose it. It is fairly safe to say that journalists and scientists have somewhat different conceptions of the nature of truth, and of the best methods of arriving at it.

In the end the *Herald* withdrew its offer (March 5, 1899); alleging, *more suo*, that Emmens' conditions were impossible to satisfy, and retreating, under cover of a great many ill-chosen words, with all the honours of defeat. I have the impression that, if it could have proved Emmens an impostor, it might possibly have brought itself to publish this fact.

In his correspondence with Sir William Crookes, F.R.S., which lasted a year (May 1897 to May 1898), Emmens afforded a good deal of information as to his personality, but much less as to his operations, and practically none as to the details of his process—which, it must be admitted, he had a perfect right to keep secret so long as he did not seek to make converts. And he did not—witness the following, in reply to an inquiry from Crookes:

“The gold-producing work in our Argentaurum laboratory is a case of sheer Mammon-seeking. It is not being carried on for the sake of science or in a proselytizing spirit. No disciples are desired, and no believers are asked for.”

Later, when the correspondence was becoming acrimonious, Crookes made use of this statement:

(*Crookes to Emmens*, 23.5.1898.)

“On the one hand you take very high ground, and affect to despise the opinions of scientific men. ‘No disciples are desired and no believers are asked for.’ . . . I am at a loss to reconcile this disdainful attitude with the eager way at (*sic*) which you snatch at every word that is said in your favour by men of the despised sect, and insist on giving to an unsuccessful experiment I was rash enough to describe to you in confidence, an importance it does not deserve and an interpretation absolutely against my explicit declaration. . . .”

The “unsuccessful experiment”* came about in this way. Emmens, in a letter to Crookes dated May 21, 1897,

* Actually, it is illogical to call any experiment “unsuccessful.” As an American professor once remarked: “Every experiment proves something. If it doesn’t prove what you wanted it to prove, it proves something else.”

indicated an experiment having a bearing on the making of "argentaureum gold." He writes:

" . . . Take a Mexican dollar, and dispose it in an apparatus which will prevent expansion or flow. Then subject it to heavy, rapid, and continuous beatings under conditions of cold such as to prevent even a temporary rise of temperature when the blows are struck. Test the material from hour to hour, and at length you will find more than the *trace* (less than one part in ten thousand) of gold which the dollar originally contained."

This, after making some further inquiries as to details, Crookes proceeded to do; and here is his account of the result:

(*Crookes to Emmens*, 22.2.1898.)

"I put about half an ounce of cuttings from a Mexican dollar into a steel mortar with a close-fitting piston, and screwed it firmly on a strong base. Over the piston I have a steel bar, 1.5 inches square, and sufficiently long to weigh twenty-eight pounds. This was provided with a collar, and was raised and allowed to drop one foot by means of a cam on a rotating shaft. It made sixty blows a minute.

The steel mortar was enclosed in a coil of pipes immersed in a water-bath, and ice was put in the bath; and through the coil of pipes a slow current of liquid carbonic acid flowed. This kept the temperature down considerably below zero F. the whole of the time. The water was hard frozen all the time of the operation. It went on for forty hours, stoppings being allowed at night (say, five hours at eight hours a day).

The results are as follows:

Hammered Mexican silver taken for assay	100·258 grains
Weight of gold bead 	0·075 grain
Equal to 	0·075 per cent.

Some of the same silver before treatment	96·837 grains
Weight of gold bead 	0·060 grain
Equal to 	0·062 per cent.

The difference is 0·013 per cent., which I consider is not enough to enable me to form an opinion on, as it is within the errors of experiment."

Such was Crookes's first attempt to repeat Emmens's production of gold from silver. He regarded it as "merely a preliminary canter," for the machinery was not working smoothly. Having overcome the defects, Crookes made a second attempt, which resulted in total failure. The reason for this, as given by Emmens, is rather curious.

Emmens was of opinion that, for his process to work at all, the silver which he used should have a suspicion of gold in it; say, 1/20,000. Crookes, who had been informed of this, but had either forgotten it or regarded it as unimportant, made his second trial with chemically-pure silver, which remained entirely unaffected by the "force-engine."

By this time the friendly tone of the Emmens-Crookes correspondence had been replaced by a pronounced subacid flavour. Crookes' experiments must have cost him a good deal, and he had, I think, come to suspect Emmens' candour. The latter, on his side, was beginning to be irritated by Crookes' apparent ineptitude and his incontestable vagueness of language. Then again, the interpretation put on the first experiment by Emmens differed widely from that of its maker. Emmens considered that, if Crookes' assays were worth anything,

they demonstrated a rise of 20·9 per cent in the gold content of the silver—from 0·062 to 0·075. He regarded this as valuable independent testimony to the soundness of his theories, and was at some pains to make the experiment generally known.

Another incident which did nothing to heal the growing breach was the publication by Crookes in the *Chemical News* (which he controlled) of what professed to be an analysis of a piece of “argentaureum” sent to him by Emmens. It ran as follows:

“A specimen of argentaureum sent me by Dr. Emmens has been examined in the spectrograph. It consists of gold with a fair proportion of silver and a little copper. No lines belonging to any other known element, and no unknown lines, were detected.”*

This was an unfortunate slip on Crookes’ part. What he had asked from Emmens, and what Emmens had sent him, was “a small piece of the *gold* you have made.” What he had examined was a sample of the product sold to the United States Mint, of which a specimen analysis is:

					<i>Parts.</i>
Gold	528·80
Silver	383·82
Copper	86·06
Platinum	0·65
Lead	0·05
Zinc	0·23
Iron	0·39
					<hr/>
					1000·00

It may be added that a much more unfortunate remark occurred in the report of an assay made for Emmens

* *Chemical News*, 3. ix, 1897.

by Preston, of the United States Mint. Argentaurum gold was stated to contain impurities of a kind "constantly present in old jewellery." I have often speculated whether the *New York Herald* ever heard of this; and, if so, what they said about it. It was as neat a way of calling Emmens a "fence" as could well be imagined.

On March 31, 1898, Emmens wrote to Crookes in a strain of dignified reproof slightly marred by a split infinitive:

"... You have made *two* experiments. In one you employed metal from a normal Mexican dollar and obtained an increase of nearly 21 per cent. in the contained gold. In the other you employed *abnormal* Mexican dollars, and obtained no gold. It seems to me that your duty is to dispassionately announce both experiments. . . ."

Crookes replied on April 30th declining further correspondence, on the ground that Emmens had violated his (implied) confidence by publishing portions of his private letters. And Emmens rejoined (May 12, 1898):

"... really, don't you think it poor sport to ride the horse of grievance? You and I are growing old, and we may surely turn our time to better account than in exchanging complaint and repartee over such a trifling matter as to whether an experiment with a bit of metal should or should not be treated as a weighty secret? . . ."

It is fair to recall that Crookes began the correspondence. On the other hand, it was chiefly Emmens who sustained it, and I have the impression that before it ended Crookes would not have been sorry if it had never started.

In a letter to Mr. H. C. Fyfe,* Emmens gave some details of the financial side of his process. By his own account, he would be prepared to convert 1,000,000 ounces of silver into 600,000 ounces of gold, for the sum of about £900,000. In other words, he could profitably charge 18s. for converting an ounce of silver, costing some 2s., into three-fifths of an ounce of gold, worth about £2 6s. In the same letter he gave some remarkable particulars of other wonders which he claimed to have accomplished:

“ . . . we have produced some remarkable allotropic forms of gold and a very singular growth of silver. We have dissolved an alloy by a mechanical method. We have rendered arsenious anhydride readily soluble in pure water. In light and radiant energy generally we are also obtaining some surprising results. And the list could be greatly extended.

“I hardly know why I tell you of these things. I think it is because you have been broad-minded enough and bold enough to acknowledge the existence of my Argentaureum Paper, No. 1, which is regarded with fear, anger, and horror by the Gravity-cum-Ether-cum-Contraction worshippers. . . .”

The paper to which he refers is entitled *Some Remarks Concerning Gravitation*,† but it might equally well have been called *Astronomical Hiccups, with Some Remarks on the Partition of Poland*, or any other name of a mixed character. Mixed it certainly is. It deals chiefly with the misdeeds of Sir Isaac Newton and his followers (why is it that the average crank generally starts by

* Published in *Pearson's Magazine*, March, 1898.

† The Argentaureum Papers, No. 1, *Some Remarks Concerning Gravitation*, etc. (New York, 1897).

picking a quarrel with Newton?) and thence passes to the old, old question of whether the Moon rotates. The reader will, no doubt, thank me for sparing him extracts from Emmens' views on this point: they are as wildly wrong-headed as those of the veriest flat-earthier. As an indication of its author's mental ability, the book is immeasurably below *Argentaurana*.

Still, crank or not, Emmens' feat of persuading the United States Mint to buy a considerable quantity of what he assured them was artificial gold has never, I think, been duplicated. Nor can I form any theory of fraud which would make such a proceeding either practical or necessary. As to the facts of the sale, there is no doubt.

Miethe. Nagaoka.

On July 21, 1924, it was announced in the Press* that Dr. A. Miethe, Professor of photo-chemistry and astronomy in the Technical High School, Charlottenburg, had obtained gold from mercury by the prolonged action of a high-tension electric current on it. The discovery was more or less accidental, and arose from an examination of the black deposit found inside mercury-vapour electric lamps in which too high a voltage had been used. This deposit was found to contain traces of gold, although the mercury had been twice distilled before being used for the lamps. Dr. Miethe made special experiments with a view to producing an appreciable quantity of the deposit under conditions precluding the accidental presence of any gold, and obtained results which satisfied him that a very minute quantity of the mercury had been actually transmuted into gold. It may be added that he held out no prospect of the operation ever becoming commercially profitable. The potential

* See, for example, the *Morning Post* for that date.

used was about 170 volts, the current being passed through the lamp (which consumed some 400–2,000 watts) for periods varying from 20 to 200 hours.

In *Die Naturwissenschaften* for July 17, 1925, Dr. Miethe gave a considered summary of his results. He claimed to have found that, in a mercury vapour lamp operated under suitable conditions, gold does make its appearance in minute but appreciable quantities. The yield, apparently, is proportional to the strength and duration of the current. Below a definite minimum voltage no gold is produced: *in vacuo* the process fails. Gold, and in some cases a metal resembling silver, can also be produced by discharges between two mercury electrodes dipping into a bath of paraffin wax.

Confirmation of these latter results was afforded by some experiments made in September 1924–May 1925 by Professor H. Nagaoka, of Tokyo. He used exceedingly high voltages, discharging through paraffin oil across a narrow gap, one electrode being iron and the other mercury. Like Miethe, he obtained minute quantities of gold, and believed that he had eliminated the possibility of accidental errors.

It has to be recorded, however, that the results of Miethe and Nagaoka are not universally accepted. The principal objection is of a most curious nature, and demands a short digression.

Believers in the possibility of transmutation, when asked to give reason for their belief, have always relied, as their sheet-anchor, upon the phenomenon of “allotropy”—the curious property which enables various of the “elements” to appear indifferently in two or more forms, possessed of widely varying properties and appearance. There are, for example, two kinds of phosphorus, yellow and red. Yellow phosphorus is highly inflammable,

easily soluble in various fluids, and extremely poisonous. Red phosphorus is not inflammable, almost insoluble, and not poisonous. Yet yellow and red phosphorus are, literally, one and the same substance; and either can be changed into the other at will. Similarly, charcoal, graphite (the core of a "lead" pencil), and diamond are all allotropic forms of one and the same element—carbon. Again, many compounds are known which, although composed of exactly the same elements in exactly the same proportions, differ widely in certain physical characteristics. This phenomenon is covered by the general term "isomerism," which, for the convenience of chemists and the bewilderment of the man-in-the-street, is divided into four branches, "polymerism," "metamerism," "stereoisomerism," and "polymorphism." Lastly, it was discovered by Soddy in 1923 that various of the elements have "isotopes;" that is to say, that there are groups of substances which answer to all the recognized tests of a given element, but differ slightly, among themselves, in their atomic weights. It is this last discovery which, some consider, stands in the way of our accepting the transmutations reported by Miethe and Nagaoka.

It was pointed out by F. W. Aston in 1925 that from the known isotopes of mercury one can deduce that gold obtained from mercury by a process of transmutation would have an atomic weight of 198 or over. Analysis of the gold obtained by Miethe, on the other hand, gave an atomic weight of 197.2 , which is exactly that of ordinary gold. Hence, it is concluded, the gold which he produced could not have been the result of transmutation.

To the plain man this appears simply paradoxical. It amounts, in effect, to saying that a thing is so obviously and demonstrably itself that it cannot be what it is. But it must be remembered that the last word has not by

any means been said on the subject. The atomic theory has already undergone such transmogrifications that its originator, Dalton, would certainly fail to recognize it in its present electronic form. Every year brings fresh emendations of last year's "epoch-making investigations," and at the present rate it may not be long before all our present notions seem, and are publicly proclaimed, as out-of-date as the "phlogiston" theory which delighted Boyle and Cavendish. For, after all, theories are nothing but efforts of the imagination, devised to help the mind to grasp the meaning of various related phenomena. They are not demonstrably or even necessarily true, and the proper way to regard them is as newspapers, to be thrown away when out of date—not as family Bibles.

One day, perhaps, we shall scrap the theory of the immutability of the elements and come, like the old alchemists, to regard them as varying forms of the same essential substance. There is nothing particularly improbable in conceiving that out of the millions of possible forms of matter some hundred or so should, by virtue of their particular structure, possess such great comparative stability that they give the impression of distinct "elements." Opinion has been veering in that direction since the discovery of radium in 1906, and "transmutation," on the electronic hypothesis, is now regarded as depending mainly upon our ability to alter the charge of energy possessed by a given atom. It would seem, however, that until some cheap source of enormous power be discovered (in which case gold would lose most of its value) the cost of the requisite energy would vastly outweigh the value of the gold or other precious metal which it produced.*

* The same applies to the many schemes which have been put forward for extracting the millions of tons of gold which the sea holds in solution.

It would almost seem, then, that we must admit the possibility of transmutation. But it is still a far cry to such transmutations as Van Helmont, Helvetius, and Price claimed to effect. It seems almost incredible that with their limited resources and empirical knowledge they should have performed feats which are still beyond us. But it is not altogether impossible. Many processes have been discovered by accident; some entirely depend upon the presence of small quantities of ingredients—catalytic agents—which remain unaltered at the end of the operation; some demand, for their success, tricks of manipulation which may easily become forgotten. What is done to-day with great difficulty and expense in a well-equipped laboratory may be a schoolboy's hobby fifty years hence—so much, most will grant. But the converse is also true: that things may have been done long ago, with very imperfect appliances, which may present, if the method of doing them has been lost, a difficult problem to the resources of the present day.

The "Philosopher's Stone" seen and handled by Van Helmont and the others, and Price's "transmuting powders," may have been vulgar tricks practised on the credulity of honest and unsuspecting men. They may also have been catalytic agents, whose nature might perhaps be rediscovered—either by direct experiment or by patient examination and sifting of the enormous extant mass of alchemical literature. The only man of modern times who explored both these avenues paid, in the end, for his temerity with his life. "Whence comes such another?"

NEW SOUTH GREENLAND

IN 1832 an American sealing captain, Benjamin Morrell,* published a book entitled *A Narrative of Four Voyages*,† in which, among other matters, he claimed to have made a remarkable Antarctic voyage in the year 1823. This narrative has long been—and still is—a stumbling-block to geographers.

The reason is twofold. In the first place, the track of the voyage is considerably at variance with accepted ideas of Antarctic geography; and, secondly, the description of it which Morrell gives, coupled with the reputation which he is known to have had among his contemporaries,‡ causes very considerable doubt to be thrown on his veracity.

Morrell's Antarctic voyage may be divided into two portions: (*a*) his passage from Kerguelen to the South Sandwich Islands in a high southern latitude; and (*b*) his exploration of the Weddell Sea, including his examination of a coast which he termed "New South Greenland," and which he placed in about 68° S., 48° W., and northward from thence to about 63° S.

* See Plate VI.

† *A Narrative of Four Voyages to the South Sea, North and South Pacific Ocean, Chinese Sea, Ethiopic and Southern Atlantic Ocean, Indian and Antarctic Ocean. From the Year 1822 to 1831.* . . . By Captain Benjamin Morrell, Jun. New York: J. & J. Harper. 1832. (Reprinted, 1841, 1850.)

‡ He was popularly known as "the biggest liar in the Pacific." Charles Enderby (head of Enderby Brothers, a well-known whaling and sealing firm) once stated publicly that "he had heard so much of him, that he did not think fit to enter into any engagement with him."

Morrell states* that he left Kerguelen, where he had been sealing in his schooner, the *Wasp*, on Saturday, January 11, 1823. He steered south-eastward until he fell in with heavy pack-ice in $62^{\circ} 27' S.$, $94^{\circ} 11' E.$ (some two hundred miles northward of Queen Mary Land).† He then hauled more to the northward, and ran eastward between about $59^{\circ} S.$ and $60^{\circ} S.$, until he reached $117^{\circ} E.$, when he again stood southward. On February 1st, he relates, he was in $64^{\circ} 52' S.$, $118^{\circ} 27' E.$ (about a hundred miles to the northward of Balleny's "Sabrina Land"), when he turned westward. Now comes the part of his account which has been most severely criticized, and which I give in full, in so far as relates to his route (pp. 65, 66.).‡

"By an observation at noon we were in lat. $64^{\circ} 52' S.$, long. $118^{\circ} 27' E.$ The wind soon freshened to an eleven-knot breeze, and we embraced this opportunity of making to the west; being, however, convinced that the farther we went south beyond lat. 64° the less ice was to be apprehended, we steered a little to the southward until *we crossed the antarctic circle*,§ and were in lat. $69^{\circ} 11' S.$, long. $48^{\circ} 15' E.$ In this latitude there was *no field-ice*, and very few ice-islands in sight. . . .

"*February 23d.*—We continued steering to the westward with every necessary caution until 4 P.M.,

* *Loc. cit.*, pp. 64–66.

† On his passage from Kerguelen to this position, he must have passed fairly close to Heard Island, which was not discovered until 1853.

‡ Dr. H. R. Mill has done the same in his book, *The Siege of the South Pole*, pp. 107, 108. It is unfortunate that his transcript begins with a misprint not to be found in Morrell—"observaton" for "observation."

Here and in the later extract I quote, verbatim, every word of Morrell's account which has any bearing upon his position and movements. I have, however, omitted extraneous passages, comments, and rhetoric, of which his book is peculiarly fecund.

§ The italics are Morrell's.



PLATE VI

CAPTAIN BENJAMIN MORRELL

*From "A Narrative of
Four Voyages . . ."*

[Facing page 256]

when being in lat. $69^{\circ} 42' S.$, we crossed the meridian of Greenwich, in a fine clear day, and with the wind from S.E. to S.S.W., from which quarter it had blown

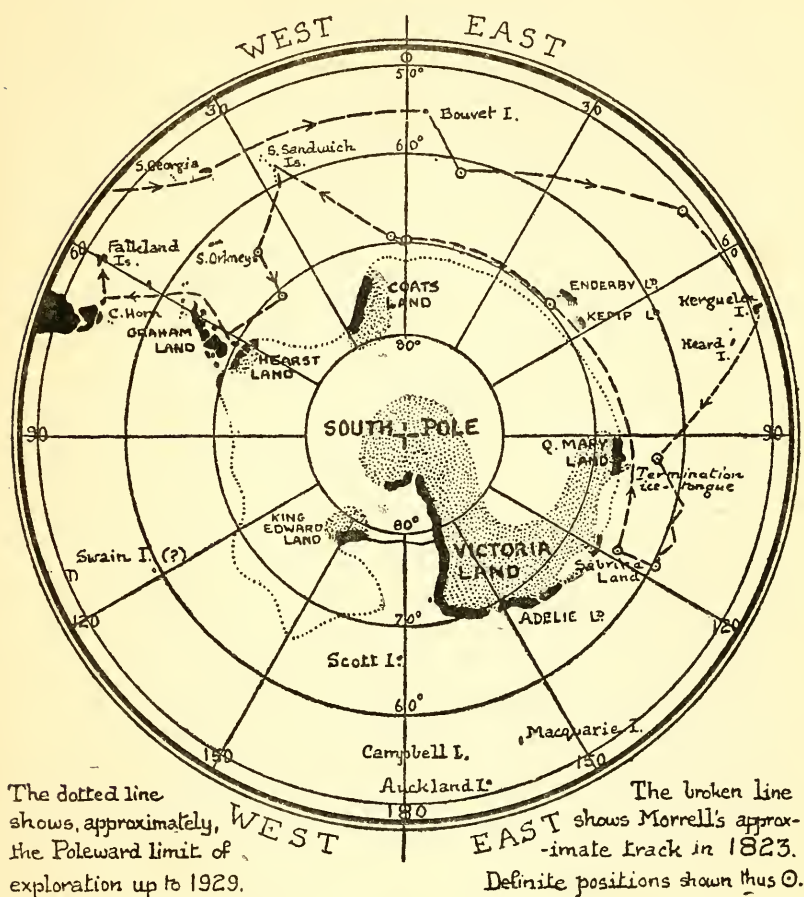


FIG. 15.—Morrell's track during his Antarctic voyage.

for the last four days. We now steered for the north and west for Sandwich Land... .

"February 24th.—Monday, the 24th of February being calm, we lowered one of our boats, and supplied

the vessel with fresh water, from the floating ice around her. We likewise tried the current, and found it setting due north, at the rate of about the eighth of a mile in an hour. . . . We had tried the current several times between the meridian of Greenwich and one hundred and eighteen degrees east; but always found it setting in the same direction, and with the same velocity. . . . Lat. $68^{\circ} 12' S.$, long. $4^{\circ} 17' W.$

"*February 25th.*—We continued our course for Sandwich Land, hoping to find among those barren islands some drift-wood, as well as seals. Fuel we were much in want of, not wishing again to cross the antarctic circle without a fresh supply. In this passage we saw in the water many seals. . . .

"*February 28th.*—On Friday, the twenty-eighth, the cheering cry of 'Land, ho!' resounded from aloft. This proved to be the Candlemas Isles, the most northerly islands of Sandwich Land; lat. $57^{\circ} 10' S.$, long. $26^{\circ} 59' W.$ These two islands are of no great extent, but one of them is of considerable height, both being burning volcanoes; and the most western having burnt down nearly to a level with the sea. We continued examining these islands towards the south, until we arrived at the Southern Thule; where, on the north-east side of the westernmost island, we found a good harbour. . . . The westernmost of the Southern Thule is in lat. $59^{\circ} 35' 10'' S.$, long. $27^{\circ} 42' 30'' W.$ "

This portion of Morrell's narrative has been bitterly and, it seems to me, somewhat unjustly criticized by many writers upon Antarctic exploration. I need only instance Commander J. E. Davis, R.N.,* and Dr. Karl Fricker.†

* See *Proceedings of the Royal Geographical Society*, 1870.

† See his *Antarctic Regions*, pp. 62-64.

The latter, in particular, dismisses it as "plausible fable." Unfortunately, in discussing it he commits such an astonishing number of blunders of all kinds as to put himself, as a critic, completely out of court. On the other hand, Morrell has not been very fortunate in his defenders, of whom the two foremost have been the late Dr. W. S. Bruce and Mr. E. S. Balch of Philadelphia. Bruce was, undoubtedly, an acknowledged authority upon Antarctic exploration; but in dealing with Morrell he seems to have allowed enthusiasm for a pet theory completely to overbalance his scientific judgment. Balch, on the other hand, writes both of Morrell and of other American Antarctic explorers with rather obvious bias and (in my submission) imperfect technical knowledge—not only of Polar exploration but of ordinary navigation.

The view expressed by Dr. Mill, in his *Siege of the South Pole*, is, in all probability, not far from the truth. He remarks (p. 111):

"... we are inclined to believe that he [Morrell] did make a passage from somewhere near the meridian of Kerguelen toward Weddell Sea at a high latitude; but on account of his blunders, his acknowledged habit of building the experience of others into his story and the silence of his contemporaries as to his exploits, we cannot credit any one of the particulars which he alleges, and must regard his claims as 'not proven.'"

I must confess that I should be inclined to go a little further than Dr. Mill towards accepting this portion of Morrell's narrative. The "blunders" of which he speaks (chiefly observations of temperature) are confined to the second portion of the voyage: Morrell generally states when he is not relating his own experiences, and no one has suggested that the voyage, if made, was really per-

formed by someone else: and "the silence of his contemporaries" proves nothing. The same might be said of every American sealer who explored the Antarctic. The only two contemporary chroniclers of their *faites et gestes* are J. N. Reynolds* and Edmund Fanning,† both of whom record Morrell's doings quite specifically.

In the light of modern knowledge of the Antarctic (still largely defective, be it noted), there is nothing intrinsically impossible in Morrell's story. The data given are scanty, but they are not inconsistent with known fact. The speed of the voyage has often been paraded as evidence of its impossibility; but in actual fact this is largely a matter of opinion. It must be remembered that the Antarctic summer of 1822-23 was, as regards ice, the most "open season" ever recorded. There was, *teste* Weddell, not a particle of ice to be seen in the southern half of the Weddell Sea; and if such conditions could obtain in what is, normally, the most ice-choked area in the world, it is not at all improbable that Morrell may have found other parts of the Antarctic, normally defended by accumulations of semi-permanent ice, in a similarly "open" state.‡

If we admit that Morrell may have been much less

* *Report . . . in relation to islands, reefs and shoals in the Pacific Ocean.* Executive Documents, 23rd Congress, 2d. Session, Doc. no. 105.

† *Voyages Round the World.* New York, 1833: London, 1834.

‡ On this point, Morrell makes two somewhat conflicting statements. He remarks (p. 67):

"I have several times passed within the antarctic circle, on different meridians. . . . While north of this latitude, say between sixty and sixty-five south, we frequently had great difficulty in finding a passage for the vessel, between the immense and almost innumerable ice-islands. . . ." implying that when inside the Antarctic Circle such difficulties were not so prevalent. But on p. 70 he says:

"From the second day after we left the 'Island of Desolation,' up to this date, March 19th, we have not passed a day without seeing fields of broken ice, or ice-islands, or both combined; and during all that period of sixty-six days, we have had, every day, more or less snow or hail."

hampered by ice than later explorers in the same regions, his runs are not strikingly fast. Between February 1st, when he was in $118^{\circ} 27'$ E., and February 23rd, when he crossed the meridian of Greenwich, he must have covered, if we accept his account of his track, some 2,750 miles, which would make his average daily distance made good about 120 miles—a mean speed of five knots when running with the prevailing wind in a presumably clear sea. The only part of his voyage to which objection can fairly be taken on this ground is his run on February 24th–28th, 1823, during which (by his own account) he covered a minimum distance of 900 miles, or approximately 225 miles a day. Still, even this makes his average speed less than ten knots, which for a short period is by no means impossible.

Unfortunately his narrative is so vague, and his recorded positions so few, that his track is hard to plot. Dr. Mill, writing in 1905, suggests that he crossed the Antarctic Circle about 115° E., and that he passed southward of Wilkes' "Budd's High Land," "Knox's High Land," and "Termination Land," and also of what is now known as Queen Mary Land. From the *Aurora's* work in 1912–13, we now know that the first three are probably non-existent, and that Morrell could not have passed southward of Queen Mary Land because that land is continental in character. But it is unnecessary, and I think unjustifiable, to assume that Morrell "steered a little to the southward" so early. From my own plotting of his track,* I suggest that he crossed the Antarctic Circle in about 78° E., in which case he would have passed about ninety miles northward of Queen Mary Land, and just clear of the Termination ice-tongue (which, however, may not have extended, in

* See Fig. 15.

his day, so far northward as it now does). If so, there is no reason to be surprised that he failed to sight any land between his farthest east and the South Sandwich Islands.

On the other hand he must, if we accept his narrative, have passed southward of Enderby Land and Kemp Land; in which case, of course, these can no longer be regarded as part of the Antarctic Continent. But I can see no improbability in the supposition. Our present knowledge of Enderby Land is confined entirely to the vague account given in 1832 by Biscoe, who was never able to get within thirty miles of it; while of the extent and appearance of Kemp Land practically nothing whatever is known—in fact, it is not absolutely certain that this is not identical with Enderby Land.* An island scarcely larger than Kerguelen would provide sufficient basis for the land seen by Biscoe; and while, from the prevailing ice conditions found in the vicinity by later explorers, it is probable that the Antarctic Continent is not very far off to the southward, it is perfectly possible that between the two a wide strait may intervene through which, in an open season, a vessel could pass without sighting land.

Morrell's account of the South Sandwich Islands affords little ground for fair criticism, and one piece of evidence is strongly in his favour. He falls, it is true,

* Kemp Land, like Biscoe's Enderby Land, has only been sighted once—by Capt. Peter Kemp, in the brig *Magnet*, on December 26th and 27th, 1833. By his account, it is the same latitude as Enderby Land, and some 200 miles further eastward. He had two chronometers (nos. 173 and 279—maker not stated) which agreed on November 27, 1833, and on January 1, 1834, but differed considerably in the interim. Off Kemp Land, he trusted to 279—173 would have placed him about 70 miles further west. He was only in command of the *Magnet* for some seven months.

From about 1850 to 1870, Admiralty Chart No. 1240 ascribed the discovery of Kemp Land to "Biscoe, December 1833," but this seems to have been merely an engraver's error.

into error when describing Candlemas Island as two islands, but here he is in good company, since both Cook and Bellingshausen made the same mistake, the latter charting it as three separate islands. It was not until 1909 that Larsen discovered it to be really a single island, whose three widely distant summits (united by a low base) appeared, at a distance, as a group of islands. Morrell's statement that he saw nine active volcanoes in the group is not in the least improbable—several of the islands are actively volcanic—and there is no warrant for Fricker's distortion of it into an assertion that all the islands were "burning volcanoes." Lastly, Morrell's report of a good harbour in South Thule—a geographical fact unknown to his contemporaries and untested till the twentieth century—is now known to be perfectly accurate.

I turn to the second or Weddell Sea portion of Morrell's narrative. As before, the relevant text is quoted verbatim.

“March 6th.—After having examined the islands of Sandwich Land, without discovering a single fur-seal . . . we again directed our attention towards the antarctic seas. At five P.M. we steered to the south and west, with the wind from W.N.W., attended with frequent squalls of snow and hail.

March 10th.—We continued standing to the south and west, with our starboard tacks on board, until Monday, the tenth of March; when, at 4 A.M., we found ourselves once more in a very dangerous situation, being hemmed in on every side by field-ice. After exerting ourselves, however, for about twenty-four hours, in a thick snow-storm, we made our escape into an open sea, entirely free of ice. This was in

lat. $64^{\circ} 21'$ S., long. $38^{\circ} 51'$ W. We then took the wind from the west, and stood to the south, under double-reefed sails, until Friday the fourteenth, when our latitude was $70^{\circ} 14'$ S., long. $40^{\circ} 3'$ W.

March 14th.—The sea was now entirely free of field-ice, and there were not more than a dozen ice-islands in sight. At the same time, the temperature both of the air and the water was at least thirteen degrees higher (more mild) than we had ever found it between the parallels of sixty and sixty-two south. We were now in lat. $70^{\circ} 14'$ S., and the temperature of the air was forty-seven, and that of the water forty-four. In this situation I found the variation to be $14^{\circ} 27'$ easterly, per azimuth. . . .*

March 15th.—After relinquishing for this season the idea of an attempt to reach the south pole, we tacked about, and stood to the north and west. On Saturday, March 15th, at 2 P.M., land was seen from the masthead, bearing west, distant three leagues. At this time the wind had hauled to the south-west, and at half-past 4 P.M. we were close in with the eastern coast of the body of land to which Captain Johnson † had given the name of New South Greenland. At 6 P.M. the wind hauled off the land, when we tacked and stood to the south, along the coast, which runs about south-by-east; our boats being out, and searching

* The omission here extends to two pages (67, 68) of Morrell's book. In them he discusses the general ice-conditions of his experience, and regrets that, being short of fuel and water, and having no scientific observers or scientific instruments on board, he felt compelled to desist from attempting, then and there, to reach the South Pole—although he felt confident that he could, at least, have got to 85° S. without difficulty. This, of course, was an over-estimate; but he might have attained, say, 77° or so. He pays a generous tribute to Weddell (who, a month earlier, had reached $74^{\circ} 15'$ S. in the same waters).

† Captain Robert Johnson, of the schooner *Henry*. He had formerly commanded the *Wasp*, with Morrell as his first mate. He left New Zealand in 1825, on a voyage of exploration between the parallels of 60° and 65° S., and was never heard of again.

the shore for seals until 9 P.M., when they returned to the schooner.

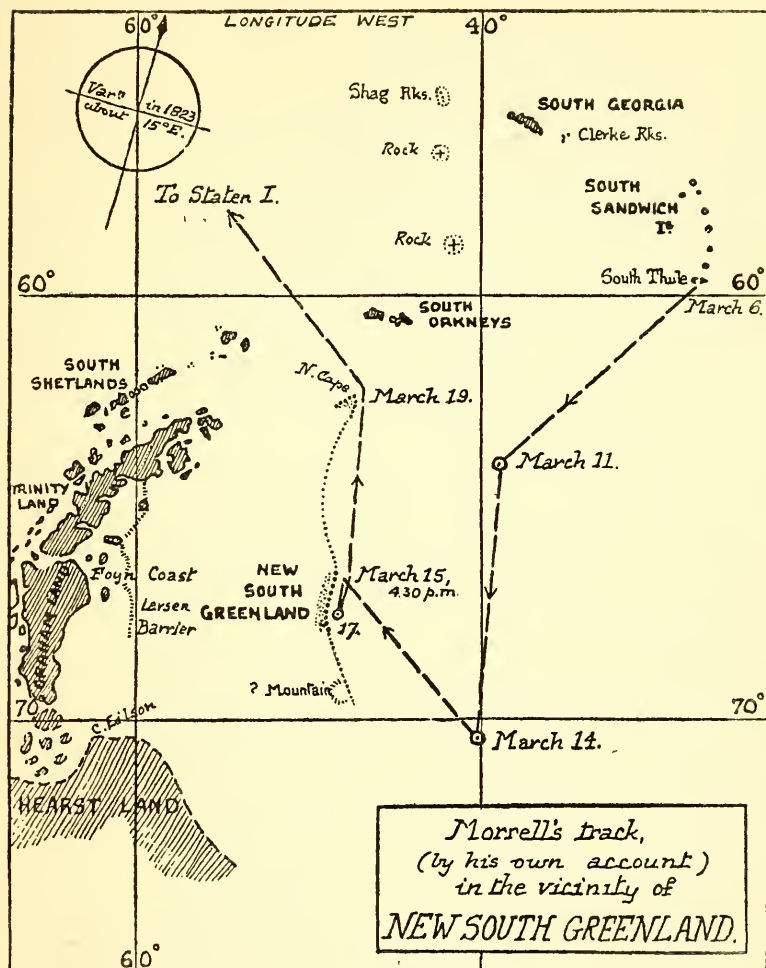


FIG. 16.—Portion of Morrell's track.

March 16th.—On the following morning, Sunday, the 16th, the boats continued their search, the vessel following or keeping abreast of them, about two miles

from the land, until the next day, at 4 P.M., when we were in lat. $67^{\circ} 52'$ S., long. $48^{\circ} 11'$ W.; at which time we took a fresh breeze from the south, attended with light snow-squalls. Variation per azimuth at 9 A.M. $16^{\circ} 4'$ easterly. The coast here tended about S.E. by S., and we thought we could discern some of the mountains of snow, about seventy-five miles to the southward.

Under the circumstances before mentioned, being without wood, on a short allowance of water, and the season far advanced, it was judged imprudent to proceed farther south; although I felt very anxious (being now in an open sea) to ascertain the extent of this land towards the south. We therefore tacked about, and stood to the north.

March 19th.—On Wednesday, the 19th, we were close in with the north cape of New South Greenland; lat. $62^{\circ} 41'$ S., long. $47^{\circ} 21'$ W. by dead reckoning, not having had an observation for three days; coast tending to the south, and S. by W. . . .

On Wednesday, the 19th of March, at 10 o'clock P.M., we bade adieu to the cheerless shores of New South Greenland, and steered for Staten Land, with a fresh breeze from the south-east. . . ."

This second portion of Morrell's voyage presents a problem which is far more difficult of analysis than any offered by his voyage from Kerguelen to the South Sandwich Islands. Doubt was cast on the existence of his "New South Greenland" as early as 1838, when Dumont d'Urville* sailed over the position of its "north cape," as given by Morrell, without seeing any indication of land. And, while it has sometimes been

* It may be noted that as a result of the same voyage d'Urville cast some very unjust doubts on Weddell's veracity.

suggested that the "appearances of land" seen by Ross in 1843 (in approximately 65° S., 49° – 41° W.) tend to confirm Morrell's statements, the two are actually, as will be shown later, in flat contradiction. No vestige of other corroborative evidence has ever been brought to light by any later explorer; while the drifts of the *Deutschland* in 1912 and the *Endurance* in 1915 have afforded ample ground for a very general and reasonable conviction that Morrell's "New South Greenland" has no real existence.

Broadly speaking, opinion on the subject may be classified as tending, wholly or in part, to one of the three following theories:

- (a) That Morrell's account of "New South Greenland" (and, in all probability, the whole narrative of his Antarctic voyage) is fictitious, or so largely so that it is not worth while attempting to extract any residue of truth from it.
- (b) That he did, actually, discover a comparatively short extent of coastline (some 50–100 miles) or a "cluster of islands," in about 67° – 68° S., $48\frac{1}{2}^{\circ}$ W.—i.e. in a position which no one else, before or since his time, has ever been near enough to sight. On this supposition, his "north cape" is given up as an illusion.
- (c) That what he actually sighted and examined was the eastern coast of Graham Land, in the vicinity of Foyen coast,* and that he explored this north-

* Both here and in Fig. 16, I have used the name "Foyen coast" for lack of a shorter term than "the northern portion of the eastern coast of Graham Land." As Mr. J. M. Wordie has pointed out (*Geographical Journal*, March 1929), Larsen applied the name "Foyen" only to a short stretch of coast slightly further northward, on "N. Graham Land"—a name which, I hope, will not be allowed to persist. It would be manifestly unjust to call the northern portion

ward to Joinville Island. On this supposition, his longitudes were some 14° in error at his southernmost point, and about 9° at his "north cape."

Of these the first supposition demands careful and unbiased consideration—a treatment which, hitherto, it does not appear to have received.

The first question which presents itself in this connection is the old tag *Cui bono?* Assuming that Morrell deliberately wrote, and sent to the printer, and allowed to go to the public, a fictitious narrative of an Antarctic voyage (of no very startling novelty, be it noted, and claiming no territorial discoveries or "world-beating" exploits) which he professed to have made some nine years previously, what good did he expect that this would do him?

I am convinced that most of the critics who hold this theory have never examined a copy of his book, but have based their judgment solely upon extracts from the Antarctic portion—which occupies seven pages out of a total of five hundred and nineteen! If Morrell wished to gain an undeserved reputation as an Antarctic explorer, one would think that he could have gone a better way about it than to bury his *pièces justificatives*, after he had forged them, in an undistinguished corner of so bulky a book.

To my mind, *A Narrative of Four Voyages . . .* is, in several ways, a remarkable work. It has many defects. It is, admittedly, a compilation. It reads like a *mélange* of Fenimore Cooper and Findlay's *Pacific Directory*. The ordinary reader, I imagine, must always have found it

of "Graham Land" by any other name than that given by the first man to sight any part of it—Edward Bransfield, Master R.N., in January 1820. He named it "Trinity Land," "in compliment to the Trinity Board."

almost unbearably dull: although precisely the same criticism would apply to the somewhat similar *Cosmos* of Humboldt—never more, I should judge, to be read by mortal. The style is simply dreadful—that of a “spread-eagle” backwoods newspaper in Andrew Jackson’s day. And in the non-hydrographic portions of the book Morrell is for ever in the limelight—attitudinizing, moralizing, shedding “manly tears,” burning with patriotic fervour, displaying his professional accomplishments (including, needless to say, bluff and “gallantry”) and bursting, on all suitable or unsuitable occasions, into floods of oratory.

But, in common fairness, there is a good deal to be said on the other side. Morrell’s faults—bombast, extravagance, and the rest—are, in great measure, the faults of his time; his contemporaries, in his own country, no doubt expected him to write like a “free-born Yankee patriot,” and would have regarded him with suspicion if he had disappointed them. But his merits are his own. I view his book as, in the main, an honest and sincere effort, undoubtedly laborious and probably ill-rewarded, to put on record a very considerable body of useful and needed information for the benefit of seamen in general. Be it remembered, too, that it was his first and, I believe, his only book. Add to this that, with the exception of Fanning and Weddell, he is the only one of the early sealers to break through their tradition of professional secrecy; that his book is very much more copious than either Fanning’s *Voyages Round the World* or Weddell’s *Voyage to the South Pole*, and contains a great deal of accurate and valuable information; that by making known his discovery of guano on Ichaboe Island he laid the foundations of a flourishing industry; and that the mere labour of putting together a five-hundred-page

book from the raw material afforded by his journals represents a most creditable piece of hard work from which very many men would have shrunk. It must, I think, be conceded that, in his way, Morrell may be said to have done a good deal to deserve well of posterity. I hold no brief for him. I think that he has himself largely to blame for the discredit into which his name has fallen among geographers: but that is a different thing from believing that such discredit was fully deserved.

I submit that those who wish to have Morrell laughed out of court as a second David Gwyn—a boasting and clumsy liar—might well be reminded that the burden of proof is upon them, and that so far they have signally failed to prove their case. For example, there is still no definite proof that land does not, as stated by Morrell, exist in about 68° S., 48° W. (although I regard this as most improbable)—for no later explorer has come within at least sixty miles of this position.* And until they can supply a plausible explanation of the motives impelling Morrell, on their supposition, to lie in print in a half-hearted and apologetic manner not in the least likely to bring him either fame or profit, it seems to me that such a theory involves bigger difficulties than those it removes.

I turn to the second theory referred to above, of which the late Dr. W. S. Bruce was the leading exponent. His views are epitomized in the following extract from his paper "The Weddell Sea."†

* Filchner, in 1912, approached this position more closely than any other explorer, in the course of a short sledge journey from the *Deutschland*. He saw no land. The *Deutschland's* drift carried her eastward of the position given by Morrell for "New South Greenland": the *Endurance*, in 1915, drifted between this and Graham Land.

† *Scottish Geographical Magazine*, June 1917.

“... I maintain that that definite and simple report of Morrell, not of a great discovery, but of his boats and ship cruising along and close to about forty miles of coast between approximately latitude 68° S. and 69° S., and very roughly in longitude $45^{\circ} 30'$ W. 48° W., with possibly a slight extension to the NW. and SE. of this for another forty miles in each direction, should not be rejected until it is absolutely disproved.”

Elsewhere in the same paper he hints that the land in question may not be continuous with the Antarctic Continent (the drift of the *Endurance*, full details of which were not then available to him, has shown that it cannot be) but may be “a cluster of islands”; while he concludes with the following remarkable paragraph, which might almost have been penned by Orontius Finæus or Martin Behaim:

“It is remarkable how Graham Land and its archipelago are topographically reflected as an inverted image of South America, and such a ‘*cluster of islands*’ would more fully complete the reflection by a balancing of the Falkland Islands ‘*cluster of islands*.’ I do not assert that there should be such a balancing, but the idea is at least suggestive, especially if the geology were also reflected, as for instance it is with the Andean Range continuing its existence in Graham Land.”

Such are the opinions of a man who, *prima facie*, was well qualified to express an opinion, as having had personal and prolonged experience of exploration in the Weddell Sea. But it will, I think, be conceded that Bruce’s concluding paragraph, quoted above, indicates a very great readiness to snatch at any evidence, however flimsy, tending to support a preconceived idea. More-

over, in his discussion of Morrell's narrative he seems to me to have drawn inferences which are not really supported by the facts.

It is literally true that Morrell's narrative speaks of "his boats and ship cruising along and close to about forty miles of coast between . . . 68° S. and 69° S." But Morrell has not written one word to suggest that this coastline stopped outside these limits, or that it was really the shores of "a cluster of islands." Anyone reading Morrell's account without any preconceived ideas, and taking it at its face value, would naturally conclude that Morrell had sailed along a continuous coastline from March 15, 1823, when he fell in with it in about 67° S., until March 19th, when he rounded its northern extremity in about 62° 40' S. If land thus described is to be resolved into a small "cluster of islands," and Morrell's "north cape" given up as an illusion, then there is much to be said for the contention that Morrell's honour would not be restored even if such a cluster of islands were hereafter discovered.

Bruce adduced the "appearance of land" seen by Ross in 1843, and long shown on the Admiralty charts in about 65° S., 47° W., as supporting his "cluster of islands" theory. But after a careful examination of his paper I am bound to confess that, to my mind, the evidence afforded by Ross' logs tells strongly *against* that theory.

According to the extracts from those logs given by Bruce, "appearances of land" were seen by both the *Erebus* and *Terror* on February 9, 10, and 11, 1843, in about 65° S. and 45°-49° W. And Bruce continues:

"Ross even indicates on his chart the possibility (in pencil notes, not confirmed by inking in) of land existing as far east as 41° W., but has evidently rejected

these observations on account of Weddell's track passing through one of the positions."

If, as I think most probable, the chart to which he refers is one of the set of track-charts of the *Erebus* and *Terror* preserved at the Admiralty, it was not actually drawn by Ross, but by J. E. Davis, then Master of the *Terror*. But, in any case, the caution shown by Ross (or Davis) in charting various phenomena as "appearance of land," and not as actual land, has been found, in later years, to have been abundantly justified; while, in at least one well-known case—the "Parry Mountains"—land definitely charted as such by Ross has gone the way of his uncle's non-existent "Croker Mountains" and other "Cape Flyaways."†

That Ross himself was not strongly imbued with the idea that land had actually been sighted in the position referred to above is evident from the fact that in his book‡ he makes no mention of it—an omission which, so far as I am aware, cannot be paralleled in the case of any other "appearance of land" known to have been sighted by him. Bruce dismisses this piece of negative evidence very lightly, merely remarking, ". . . Ross' omission in the text of his popular narrative is evidently merely a slip." But I do not think this is an adequate explanation, even if we concede that Ross' ponderous

* The "Parry Mountains" were charted by Ross as a chain extending southward from Mt. Erebus. Their existence was conclusively disproved by Scott in 1902. It is only fair to point out that Ross was never near enough to discover his mistake.

† Sir John Ross (uncle of the more famous Sir James C. Ross) "discovered" and charted, in 1818, a chain of mountains, which he named the "Croker Mountains," extending right across Lancaster Sound. Parry (Ross' second-in-command), who had seen nothing of them, although in company with Ross, sailed (in 1819) right over their charted position.

‡ *Voyage of Discovery and Research in the Southern and Antarctic Regions . . .* London, 2 vols., 1847.

and little-read account of the voyage—the only one, be it noted, that he ever published anywhere—can fairly be described as a “popular” narrative. It seems far more likely that Ross, noting that Weddell’s track passed through one of the several similar “appearances of land” seen on February 9–11, 1843, concluded that this, *like the remainder*, was an illusion due to peculiar conditions of ice and atmosphere.

Just as it is an “illicit process of the major” to select portions of Morrell’s “east coast of New South Greenland” for credence and reject the remainder, so is it unfair both to Ross and to the man-in-the-street to retain some of his “appearances of land,” and to discard those which have been disproved. All must stand or fall together. There is nothing whatever in the evidence afforded by the logs of the *Erebus* and *Terror* enabling us to say that the “appearances of land” seen in 49° W. (which have not since been examined) differed in any way whatever from those in 44° W. (which were sailed over by the *Deutschland* in 1912) or those in 41° W. (which had been sailed over by Weddell in 1823). The only reasonable conclusion is that all, alike, were non-existent. And when we note that Ross’ track in 1843 *runs straight through* the coastline of Morrell’s “New South Greenland,” intersecting it at right angles in about $64^{\circ} 30' \text{ S.}$, $47^{\circ} 30' \text{ W.}$, it is difficult to understand how the one could ever have been adduced in support of the other.

Mr. E. S. Balch, the other leading defender of Morrell, is inclined, in his *Antarctica*,* to adopt the third supposition; namely, that what Morrell actually sighted and examined was the eastern coast of Graham Land, in the vicinity of Foyn coast. He remarks:

* *Antarctica*, by Edwin Swift Balch; Philadelphia, 1902, pp. 100–107.

“There may be a land in 67° south latitude, 48° west longitude, for no one but Morrell claims to have been in that position, but there may also have been a mistake in Morrell’s published longitudes, and this may have arisen in one of two ways. He was a sealer, and it is not likely that he had a chronometer or even an ordinary watch which was reliable, and he therefore might easily have made, and probably he did make, errors in his observations. Again, his book is a bad, cheap and careless piece of typography, and the figure 4 may simply have been substituted for the figure 5: 48° for 58° .”

If Mr. Balch, in the paragraph quoted above, had also suffered at the hands of the printer, and had actually written “There may be *land* in 67° south latitude . . .,” and not “ . . . *a land* . . .,” no exception could be taken even at this date (he wrote in 1902) to his statement: but it is perfectly certain that there is not, in that position, any such extensive coastline as Morrell describes. With regard to his remarks on Morrell’s possible errors in longitude, it is suggested that these are somewhat cursory.

It is true, in the first place, that Morrell does not directly state, in his account of the voyage, that he possessed a chronometer; and it is also true that it would be a fair inference, from the general habits of the American sealers of his time, and from his remark* with reference to proceeding farther south from his southernmost point, that he was “ . . . destitute of the various nautical and mathematical instruments requisite for such an enterprise . . .,” that he did not carry one. But, in this event, what are we to make of his account of

* On p. 67 of his book.

his search for the Aurora Islands, which he thus describes:*

"On the 6th [November 1822] we crossed the spot which the Aurora Islands were supposed to occupy, without meeting any indications of land. After running to the east, in the parallel of $52^{\circ} 45' S.$, as far as $43^{\circ} 50' W.$, we stood to the north, to latitude $52^{\circ} 30'$, when we took the wind from south-east, and made a west course, keeping in the last parallel, by double altitudes, every opportunity, both morning and evening, and meridian altitudes of the moon and different planets. We continued making a west course until we were in long. $50^{\circ} 22' W.$, when we steered to the south until we were in latitude $53^{\circ} 10'$, and ran down in that parallel to the long. of $40^{\circ} 0' W.$. . ."

No one in his senses would even speak of having carried out a search on this plan without the assistance of a chronometer. Moreover, Morrell gives his position, in latitude and longitude, both in the passages quoted and in many other portions of his account of the voyage, as "by observation"; on March 19, 1823, he goes out of his way to remark "... long. $47^{\circ} 21' W.$ by dead reckoning, not having had an observation for three days."† If we attach any degree of credence to his narrative, we must conclude either that he carried a chronometer or that he was an absolute expert at the taking of lunar distances at all times and in all circumstances. It is needless to point out which is the more likely supposition; and, in either event, he should not have made a mistake of 14° in longitude during the ten days which elapsed between his leaving South Thule (whose longitude he gives correctly enough as 27°

* *Loc. cit.*, pp. 56-57.

† P. 66, *ibid.*

42' 30" W.), and his falling in with the coast of "New South Greenland."

I say 14°, because the Foyn coast is in approximately 62° W., not 58°. If, then, we accept Mr. Balch's typographical error, we must combine it with a further error of 4° arising from some other cause. But, as I have already stated, there are objections even to this. I do not profess to have checked every one of the very numerous positions given by Morrell in his book; but I have no hesitation in saying that if it is a "bad, cheap, and careless piece of typography," the standard of American nautical works in 1832 must have been vastly superior to that of the similar works produced in any other country then or since.

Moreover, no simple single correction will, as imagined by Mr. Balch, remove the difficulties attending Morrell's longitudes. He places part of his "New South Greenland" in 67° 52' S., 48° 11' W.; thus, if what he really saw was the Foyn coast, making an error in longitude of about 14°. But he also places the "north cape of New South Greenland" in 62° 41' S., 47° 21' W. (D.R.). On the previous supposition—or on any other, assuming that it was really land he saw—this can only have been the north point of d'Urville I., which is in about 62° 55' S., 56° 10' W. His error in longitude is thus reduced to about 9°. Furthermore, whatever his absolute error in longitude, he makes the northern extremity of his land bear roughly 005° from the southern, while actually this bearing is about 035°; a blunder not to be overcome without a most lavish scattering of assumed errors in typography.

If it were not for this, and for an even more inexplicable discrepancy noted later, the theory that what Morrell visited was the Foyn coast has much to commend it.

Until a few months ago, it had more. A glance at a modern chart of the vicinity, such as Admiralty Chart No. 3175, will show that the Foyen coast agrees very closely in extent and orientation with the stretch of coastline which Morrell claims to have searched for seals. As charted by Larsen,* it runs southward, slightly inclining to the westward, from 67° S. to a little beyond 68° S. Morrell's coastline does precisely the same. He coasted down it for about fifty miles or so to $67^{\circ} 52'$ S., and described it as running about south by east. He states, by the way, in the "Advertisement" to his book that "... all the courses and bearings in the work have been made by *compass* . . .": and he records that the variation in this instance was $16^{\circ} 4'$ E. "per azimuth at 9 a.m." The orientation of his coast therefore agrees precisely with Larsen's; while the latter's charting stops just beyond 68° S., and in no way controverts Morrell's statement that he observed it to trend thence south-east by south for some seventy-five miles farther.

Again, the Foyen coast, as charted by Larsen, has its northern extremity in about 67° S., and further northward of it he found a wide bay, filled by an ice-barrier. Morrell says nothing specifically as to the nature of the coast northward of 67° S., which is precisely what one would expect if he had found it receding to the westward, ice-fronted, and unsuitable for sealing.

Quite recently, however, the charting of the Foyen coast has been largely amended as the result of Sir Hubert Wilkins' great flight from Deception Island to Hearst Land and back. Such of his provisional results as I have seen (notably a map published in the New York *Geographical Review*, July 1929, which I have followed in Fig. 16) differ markedly from Larsen's

* And later corrected for longitude by Nordenskjöld.

charting, and proportionately diminish the resemblance between "New South Greenland" and the Foyen coast. They do not, however, by any means destroy it.

In any case, the theory that what Morrell saw was the Foyen coast is open to a grave objection; one to which, so far, insufficient attention has, I think, been given. It arises from a consideration of his movements, as reported by himself in his book, between March 6 and 15, 1823.

On March 6th, by his account, he was at the South Sandwich Islands and, apparently, at South Thule.* At 5 p.m. on that day he steered "to the south and west, with the wind from W.N.W. . . ." and after being beset on the 10th, escaped into open water on the 11th, being then in $64^{\circ} 21' \text{ S.}$, $38^{\circ} 51' \text{ W.}$, and having run some 400' in three and a half days. This position agrees very well with what he tells us of the wind, if we assume that he was close-hauled on the starboard tack.

He goes on, "We then took the wind from the west, and stood to the south, under double-reefed sails, until Friday the 14th," when he reached his furthest south, $70^{\circ} 14' \text{ S.}$, $40^{\circ} 03' \text{ W.}$ Here, again, we find him where we might expect him to be; having run some 370 miles in three days, still on the starboard tack.

But then, by his account, "relinquishing for this season the idea of an attempt to reach the south pole, we tacked about, and stood to the north and west," the wind having backed to south-west, and the very next day, Saturday, March 15th,† at 4.30 p.m., he is close in with the coast of "New South Greenland," in about 67° S.

* See Fig. 16.

† This was actually the 14th, as Morrell states that his dates are in nautical time. The elapsed periods are, of course, unaffected.

Now, if this was actually the northern end of the Foyen coast, it is necessary to postulate that the position which he gives for his furthest south (a position which, one imagines, would have been specified more carefully than any other in the course of the voyage) must be grossly incorrect. Taking it as it stands, he must have run some five hundred miles in a little over twenty-four hours, and on a course within four and a half points of the wind. *Per contra*, on the assumption that there really was such a coast-line as he described in about 67° S., 48° W., there is nothing so very improbable in the course and distance run—some 250 miles on a course about seven points off the wind.

Matters are not much bettered wherever we place him at his furthest south. If we plot the run which he claims to have made on the 14th–15th backwards from the northern extremity of the Foyen coast, the position of his furthest south becomes about $70^{\circ} 14'$ S., 53° W. To get him there from South Thule, it is necessary to assume that he ran distances about 25 per cent. greater than he states, and that he laid up very close-hauled indeed—two assumptions which are mutually incompatible. Incidentally, we must jettison not only his own “furthest south” position, but also that which he gives for March 10th–11th.

There is, however, one avenue of escape from this dilemma. We may, if we please, assume that while beset on the 10th–11th he was actually set a very considerable distance to the westward—a set which, owing to the bad weather then prevalent, he had no means of detecting. On the other hand, this set must have been due to current, since it would have been more or less dead to windward; yet its direction is at right angles to the drifts of the *Deutschland* and *Endurance*.

Whatever we assume or reject, two prominent facts emerge. Morrell's account of his courses and distances run between South Thule and "New South Greenland" fits in well with the theory that he actually fell in with a coast-line in about 67° – 68° S., 48° W., but this theory appears, on most weighty grounds, to be untenable. On the other hand, Morrell's account of "New South Greenland" fits in well with a known stretch of coast-line in the same latitude, only further west—the Foyt coast—but this theory is difficult to reconcile with the courses and distances run from South Thule.

So far, I have done my best merely to summarize and present the data in connection with Morrell's Antarctic voyage. In what follows I give my own opinions for what they may be worth.

I suggest that Morrell has had something less than justice done to him hitherto. He may have been a braggart and a boaster, but there is no evidence that he was a deliberate liar, or that in his account of his Antarctic voyage he was not doing his best to set down a truthful account of what he did and where he went. As regards the first portion of his voyage—from Kerguelen to the South Sandwich Islands—there is nothing in his account to which, in the present state of our Antarctic knowledge, exception can fairly be taken. With regard to his "New South Greenland," the case is otherwise; but the balance of evidence, I suggest, goes to show that it is, actually, identical with the Foyt coast.

There is, it may be pointed out in conclusion, an excellent way in which future Antarctic explorers can test Morrell's veracity. If, at some future date, Enderby Land is found to form part of the Antarctic Continent, Morrell's most inveterate champions will, perforce, have to throw up the sponge; if, on the other hand, it is

found to be an island, he will no doubt receive some measure of tardy justice. Meanwhile, we can at least be charitable to the memory of a man who has, I suggest, received far more than his due share of posthumous defamation.

THE CANALS OF MARS

. . . *But who shall dwell in those worlds, if they be inhabited? Are we, or they, Lords of the World? . . . And how are all things made for Man?*

To these questions, asked long ago by Kepler, there is no ready answer. Nor, indeed, is the need of an answer very apparent; they bear a family likeness to the irritating conundrums of which the Book of Job is full. With most of us, the living of our own lives gives us more than enough to worry about—and if we take an interest in the lives of others (outside our own immediate circle), it is fairly safe to assume that they are persons of some notoriety; and, at the very least, that they are, or have been, co-inhabitants of our own little planet.

Some, indeed (chiefly eminent persons of advancing years, duly impressed with a proper sense of their own importance), have stoutly denied the possibility of there being any form of life in any other world than this. Thus in the 'fifties of last century William Whewell, Master of Trinity, published a work entitled *Of the Plurality of Worlds*,* for which a better title would have been "*The Creator's Power Limited to this Earth, and the Reasons. By One on the Steps of the Throne.*" As has been said, it seemed planned to demonstrate that

. . . throughout all Infinity
There is no one so great as the Master of Trinity.

* *Of the Plurality of Worlds: An Essay.* (Anonymous.) 1853.

It stung Sir David Brewster into the composition of an almost equally bizarre reply,* *More Worlds than One, The Creed of the Philosopher and the Hope of the Christian*. The antagonists were well matched; it is difficult, even after reading their works with close and painful attention, to discover which should be awarded the palm of ignorance in matters astronomical. Whewell was the more dogmatic; on the other hand, Brewster laboured under the handicap of possessing a firm belief in the literal inspiration of Holy Writ. Peace to their ashes!

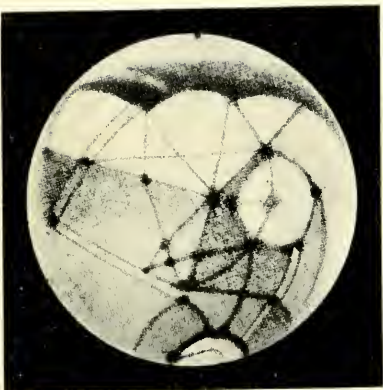
In much more recent times the late Dr. Alfred Russel Wallace published a work† in which he attempted to show that "the earth is the only inhabited planet, not only in the Solar System, but in the whole stellar universe." It is fair to add that he was eighty when he wrote it. His preface breathes a spirit of serene and inextinguishable self-satisfaction:

"Having long been acquainted with most of the works dealing with the question of the supposed *Plurality of Worlds*, I was quite aware of the very superficial treatment the subject had received, even in the hands of the most able writers, and this made me the more willing to set forth the whole of the available evidence—astronomical, physical, and biological—in such a way as to show both what was proved and what suggested by it.

"The present work is the result, and I venture to think that those who will read it carefully will admit that it is a book that was worth writing. It is founded almost entirely on the marvellous body of facts and conclusions of the New Astronomy together with those

* Published in 1854.

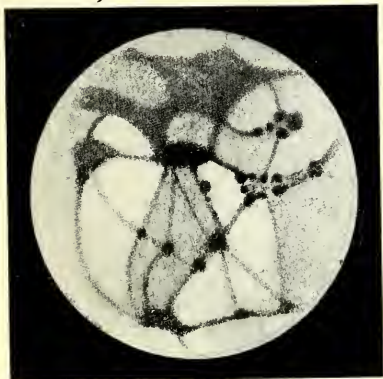
† *Man's Place in the Universe*, Alfred R. Wallace, LL.D., D.C.L., F.R.S., etc.; London, 1903.



1920. MAGGINI (181°)



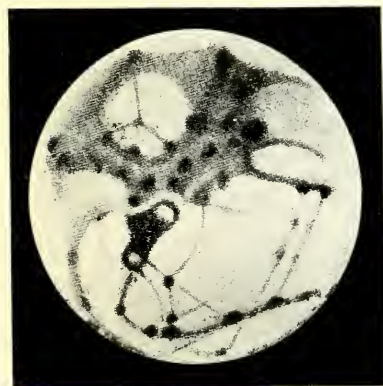
1920. PHILLIPS (182°)



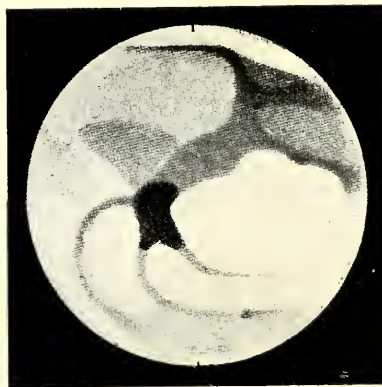
1922. MAGGINI (58°)



1922. PICKERING (58°)



1922. MAGGINI (300°)



1922. PICKERING (399°)

PLATE VII DRAWINGS OF MARS, 1920-1922

By Dr. M. Maggini, the Rev. T. E. R. Phillips, and Dr. W. H. Pickering

The figures in brackets show the meridian longitude of the centre of the disc, which, it will be noticed, is practically the same for each horizontal pair
The 1922 Maggini drawings were made with slightly higher powers, and better conditions of visibility, than the remaining four

*Reproduced by courtesy of
Dr. W. H. Pickering*

[Facing page 284

reached by modern physicists, chemists, and biologists. Its novelty consists in combining the various results of these different branches of science into a connected whole, so as to show their bearing upon a single problem—a problem which is of very great interest to ourselves.”

Actually, the problem raised by Wallace’s unfortunate book was this. When an eminent scientist, in his dotage, rushes into print upon a complicated subject which he is no better qualified to discuss than thousands of other people, and endeavours to prove a universal negative by a jumbled mass of theories and opinions selected (according to his preconceived notions) from various conflicting authorities, what inference are we entitled to draw as to the probable value of his work upon his own special subjects?

Moreover, the question of whether any form of life as we know it exists in other worlds is not merely complicated—it is one which cannot be solved by any amount of *a priori* reasoning. The conclusions reached in this way are conditioned entirely by the assumptions which one chooses to make.

If one adopts the attitude that life can only exist under conditions closely analogous to those which obtain on the earth, then it is not difficult to make out a strongly reasoned case for supposing that such conditions are not exactly paralleled anywhere else in the Solar System—or, for that matter, outside it. Such is the method of Whewell, Wallace, and others.

On the other hand, if we can bring ourselves to believe that life is still possible in conditions—such, for example, as a total absence of air, or of water; or temperatures ranging from (say) red-heat to something approaching

the absolute zero—which would be immediately fatal to anything which we should regard here as “living,” then there is no difficulty in concluding that

... there's not the smallest orb which thou beholdest which is not densely populated. Such was the method of Brewster, and by its aid he concluded that not only the stars and planets, but also the airless moon, and even the sun, were to be regarded as the abode of life. It is only fair to add that a very great astronomer indeed, Sir William Herschel, held the same view with regard to the sun.

On such a subject, then, theory is an entirely untrustworthy guide. But direct observation is scarcely better off. The largest and most powerful telescope yet constructed, the Mount Wilson 100-inch reflector,* working under the most favourable atmospheric conditions, would scarcely enable an observer actually to see living beings (of any ordinary dimensions) in a body so near to us as our own moon. Such feats are reserved for instruments such as the telescope (apparently of the Rosse type) erected by the Gun Club of Philadelphia on the summit of Long's Peak, Rocky Mountains:† or that alleged to have been set up by Sir John Herschel at the Cape in 1836,‡ whose powers were so miraculously assisted, in flat defiance of all principles of optics, by a “transfusion of light through the focal object of vision,” and whose

* I understand that a 200-inch telescope of this type is at present under consideration. For planetary observations, however, a large reflecting telescope is not greatly superior to a smaller achromatic, definition in such cases being more important than light-gathering capacity.

† According to Jules Verne (*Hail, friend of my youth!*) in his *From the Earth to the Moon*.

‡ The reference is to the once-famous *Lunar Hoax*, published, as serious truth, by R. A. Locke in 1836. It was cleverly put together and, up to a point, convincingly written. The book is very scarce, but a fairly full discussion of it may be found in any complete edition of Poe's works, as an appendix to his *Journey of Hans Pfaall*.

reported disclosures of lunar sheep, bat-winged savages of amorous inclination, and tropical vegetation were, for some time, greedily swallowed by many people who ought to have known better.

If, then, analogy gives no sound basis and direct observation fails, must we conclude that there is no way by which we can come to any conclusion as to whether life exists in other worlds? By no means. We may, if we wish, inquire of "the spirits." Here are some "revelations" upon the subject, supposed to have been communicated by Sir Walter Scott * (of course, he would know) at a *séance* held in London on June 27, 1895. The medium was the celebrated Mrs. Piper, who professed to call up Scott at the request of Professor W. R. Newbold. Sir Walter is describing the sun.

"Well, now we move on towards this fire, now reach its borders, and notwithstanding the extreme heat we pass through it, and we find ourselves upon a solid bed of hot clay or mud. This is caused by gravity. Understand where we are; we have now reached the limit;† we find it very warm and deserted, like a deserted island. . . . Now we see what we term monkeys, dreadful-looking creatures, black, extremely black, very wild. We find they live in caves which are made in the sand or mud, clay, etc. Now, sir, for that I will be obliged to discontinue our journey until some future time."

At a sitting next day the medium, who may have tried to learn a little astronomy in the interval, had another

* One wonders why they did not call up Brewster; or, for that matter, Newton, or Herschel, or somebody who might be expected to have better qualifications. In the spirit world, apparently, celebrities, although obviously suffering from softening of the brain, are always ready on tap.

† So one would suppose; but the credulity of "spiritualists," unfortunately, is infinite.

shot, and attempted to explain that the scene described had been on the earth, not the sun. However, worse was to come.

"PROF. NEWBOLD. What are the sun spots?

"SCOTT. This is the shadow of the earth, Sir.

"PROF. NEWBOLD. You are thinking of eclipses. I understand this, but I mean the black spots sometimes seen on the sun?

"SCOTT. Oh, I beg your pardon, Sir; I did not understand your question-thoughts.

"PROF. NEWBOLD. I beg your pardon.

"SCOTT. No, Sir; I understand now: the spots on the sun are . . . yes, Sir . . . are the so-called satellites which surround it; this produces a dark mass of spots."

On being asked about the climate of Mars, he replied, "Very fair: it is in the torrid zone!"

But, apart from these imbecilities, there is certainly one way in which we might be able to obtain evidence pointing to the conclusion that another heavenly body was inhabited. Our present instrumental resources are sufficient to let us scrutinize the nearer planets fairly closely; and if we detected on them any structures of an unmistakably artificial character, it would be a fair proof that intelligent beings existed there. By "structures," I do not mean ordinary houses. But if, say H.M.S. *Hood* were suddenly transported to the surface of the moon, she would probably be detected by the Mount Wilson Observatory, to the great surprise of its Director and the no small consternation of Their Lordships the Lords Commissioners of the Admiralty.* And,

* We must, however, remember the saying: "There is nothing the Navy cannot do. I do not think that the Admiralty have ever contemplated operations

if the Panama Canal were transferred there, and filled with water (there is none in the moon) the same telescope would probably show it in its whole length, and would certainly pick up the Culebra Cut and the Gatun Locks. Indeed, it was suggested many years ago, by (not unnaturally) a German mathematician, that we should attempt to communicate with the planets by outlining with lights, in some such place as the Sahara or the Russian steppes, geometrical figures (such as Euclid I. 5 or I. 47)* of colossal size. The proposal, however, met with no response. Its utility was highly questionable, while its probable cost was sufficient to appal the most stout-hearted member of the I.L.P. (which, however, was not then in existence).

Now it so happens that on one of the nearer planets, and the one whose surface can be most easily observed,† many observers believe that they have detected structures of an artificial character—the celebrated Martian “canals.” If we accept their observations and deductions, we must conclude that Mars is covered by a fine network of artificial canals, the work of creatures of a very high order of intelligence and possessed of engineering abilities and resources much in advance of our own. These resources they have devoted to what must be the greatest engineering work ever accomplished—an irrigation system designed to conserve and distribute their ever-dwindling

in the Moon; but it is a fact that in 1812 they proposed to send a frigate up the Falls of Niagara for the purpose of reinforcing our squadron on the Great Lakes.

* The famous “Asses’ Bridge,” and the equally famous theorem that the square on the hypotenuse equals the sum of the squares on the other two sides. I am not sure which of the eighty-odd known proofs of this proposition it was intended to employ.

† The only nearer planet at any time (except the asteroid Eros) is Venus, with a cloudy atmosphere, and never well-placed for detailed observation. It is fairly certain that the giant planets—Jupiter, Saturn, Uranus, and Neptune—are worlds in the making, which are gradually cooling and consolidating. Mercury is subject to the same observational obstacles as Venus.

water supply; a standing monument of a magnificent resistance offered by intelligence to the cruel fate which must, ultimately, overtake life in our own planet, and which has pressed more insistently upon the Martians because, so far as we can judge, their planet is both smaller and older than ours.

It is a fascinating theory, which admits of being worked out in great detail. For example, Mr. C. E. Housden, in a work published in 1914,* has gravely discussed the hydraulic problems of the hypothetical Martian canal system. His monograph would, no doubt, be of considerable assistance to an engineer who contemplated applying for a post in this undertaking. But it is open to one grave objection, which is of a rather formidable character. No clear and unquestionable proof has yet been given that the Martian canals—understanding by that word artificial structures of any kind—really exist.

It may be as well to recall a few facts about Mars itself. It is much smaller than our earth, having a diameter of a little over 4,000 miles.† Like ourselves, it revolves around the sun, but further off—some 50,000,000 miles further—and takes nearly two of our years to complete its circuit. About every two years and fifty days, on the average, Mars and the earth are in opposition—that is to say, in line, or practically so, viewed from the sun: but Mars' orbit is so eccentric‡ that the actual dates of these oppositions—which are the most favourable opportunities for examining the surface of Mars—and the distance between the planets on such occasions, vary

* *The Riddle of Mars the Planet*, C. E. Housden, London, 1914.

† About 4,250 miles.

‡ It was this eccentricity—greater than that of any other planet except Mercury—which induced Kepler to discard the old theory that the planets revolved in circles—a step which led him to discover his famous three "Laws."

considerably. In the most favourable circumstances, Mars' distance when in opposition is about 35,000,000 miles.

Now, as astronomical distances go, 35 million miles is not very much: it is, for example, less than half the distance from the earth to the sun, while in comparison with the distance of some of the nearest stars it is, as Sir Boyle Roche once said (or did not say), "a mere flea-bite in the ocean." But, at the same time, it militates very seriously against our examining the surface of Mars in detail. There are comparatively few astronomical telescopes now in existence which give as good a view of Mars, even when in opposition, as can be obtained of the full moon with the naked eye:* and quite an ordinary single-draw hand telescope will give a better view of the latter than can be got of Mars with any telescope yet constructed.

The earliest telescopic observations of Mars (made with telescopes of small power) revealed, so far as can be judged by the drawings which have survived, very little real detail;† and Sir William Herschel's drawings, made with a large reflector not very well adapted for planetary work, have been described as caricatures. Beer and Madler (1830-37) produced the first reasonably accurate chart of the planet's principal features.

By the middle of the nineteenth century, it was generally agreed that Mars exhibited snow-caps at its poles which almost entirely disappeared, alternately, during the Martian summer of their respective hemispheres;

* In other words, the apparent disc of the planet looks considerably smaller than a threepenny-bit held at arm's length.

† It should be gratefully remembered, however, that drawings made by Hooke (1666) and Huyghens (1672) have proved most serviceable in determining Mars' rotation-period, which is practically the same as that of the earth (actually, 24h. 37m. 22.67 secs.).

that it had seasons resembling our own; and that on its disc could be traced large areas of fluctuating outlines, some being of a reddish-yellow colour, while others were darker, appearing of a neutral tint to some observers and a greenish to others. It was assumed that the former portions were land and the latter (which seemed to be united in various places by narrow straits) seas. Such, in general, was the state of areography (the mapping of Mars) at the time of the memorable opposition of 1877.

It was memorable for two reasons. Mars had for long been regarded as possessing no satellites,* except in the imagination of Swift† and Voltaire‡ (both of whom had stated, long ago, that it had two). But on August 11-17, 1877, Asaph Hall, working with the Washington 26-inch refractor, succeeded in detecting two tiny attendants of Mars, which he very aptly named Deimos and Phobos.§ They are absurdly small, Phobos being about a dozen miles in diameter, and Deimos about half as much: but, if small, they move quickly. Deimos makes his circuit in about thirty hours, and (the Martian day being much the same length as ours) stays in the sky for days at a time; while Phobos presents the unique spectacle of a satellite revolving round its primary more than three times as fast as the latter rotates, so that it rises in the west and sets in the east.||

It was during the same opposition that G. V. Schiaparelli,¶ of Milan, discovered a number of fine

* Tennyson, for example, sang of "... the snowy poles of moonless Mars." I believe he altered this, later, to "... the snowy poles and moons of Mars."

† In *Gulliver's Travels*.

‡ In *Micromegas*.

§ Fright and Panic—the Homeric names of Mars' chariot-horses.

|| During Scott's second Antarctic expedition, one of his parties had the curious experience of seeing the sun set in the morning and rise the same afternoon. They were a little west of 180°, and kept G.M.T.

¶ Born 1835; Director of the Milan Observatory 1862-1900; died 1910. During the last years of his life he was totally blind.

streaks on the Martian disc which had not been charted by any previous areographer. These he termed "*canali*"*—"channels"; a perfectly suitable term, not implying any artificial origin. That other observers, with better instruments than Schiaparelli's $8\frac{1}{2}$ -inch refractor, should have failed to notice these "canals," as they rather unfortunately came to be called, seemed a little surprising, but it is now known that, whatever the nature of the phenomenon, most of the canals are best seen at oppositions when Mars is not very favourably placed for observers in the higher northern latitudes.

A few of Schiaparelli's canals were confirmed by old drawings made by such observers as Dawes and Green, on which indications of them could be traced; but most of them had to wait for even a limited acceptance until 1886, when they were independently† observed by Perrotin and Thollon with the Nice 15-inch telescope. In the interim, it was generally supposed that Schiaparelli had been the victim of an optical illusion or of an unusually vivid imagination; and support seemed to be lent to this view when he announced that at the opposition of December 1881, he had observed that as many as thirty of the canals which he had previously observed as single streaks had now proved to be *double*, the two streaks running parallel. In certain cases a canal was seen as single one night, and as double the following night; while the distance apart of the pairs amounted, in extreme cases, to as much as 500 miles.

However inexplicable, this duplication was also confirmed by Perrotin and Thollon, and it gradually came to be accepted that the canals were not figments of Schia-

* The term had been used, in like manner, by Secchi in 1859.

† So we must believe: but the canal-chart produced by the Nice observers is almost indistinguishable from Schiaparelli's own (which was available to them) and differs widely from more recent observations.

parelli's imagination, and that the failure of observers in general to see them was due simply to the fact that they lacked one or more of three essential requisites which the Italian astronomer possessed—a good telescope, great skill in observation, and favourable atmospheric conditions. Opinion swung round, and before very long observers were competing as to the number, size, and extent of the single and double canals which they could find, and make room for, on their Martian charts (see Fig. 17).

Easily the most prominent of these observers was the late Dr. Percival Lowell. Before he erected his famous observatory at Flagstaff (Arizona), Lowell visited France, Algeria, and various American sites, always looking for clear conditions of atmosphere. As he himself remarked:

“A steady atmosphere is essential to the study of planetary detail, size of instrument being a very secondary matter. A large instrument in poor air will not begin to show what a smaller one in good air will; when this is recognized, as it eventually will be, it will become the fashion to put up observatories where they may see rather than be seen.”

He began his life-work at Flagstaff with an 18-inch refractor, to which a 24-inch refractor and a 40-inch reflector were afterwards added. Certainly, as regards instrumental equipment and favourable conditions for observation, his observatory had, and has, few rivals.

Lowell's results, which he embodied in a large number of scientific papers issued as periodical *Lowell Observatory Bulletins*, and also in three popular works,* were of a very surprising kind, and it is not surprising that few were found who could accept them in their entirety.

* *Mars*, 1895; *Mars and its Canals*, 1906; *Mars as the Abode of Life*, 1909.

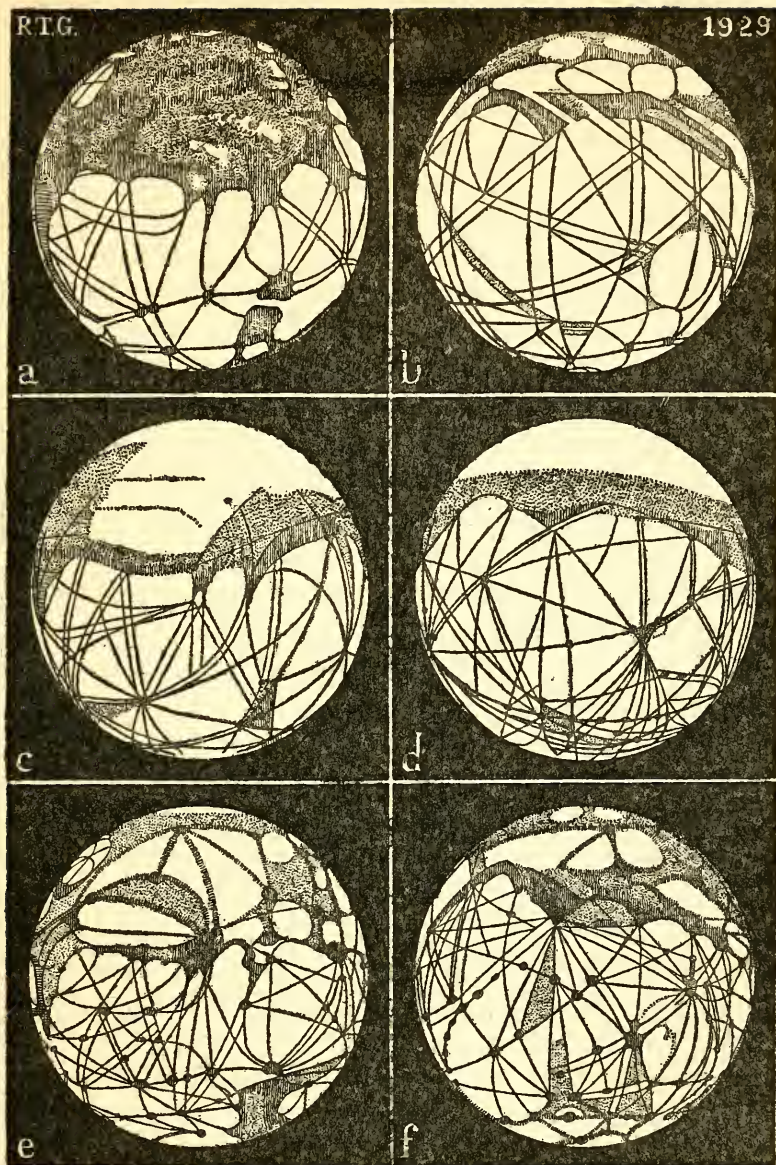


FIG. 17.—The Canals of Mars, as mapped by three of their most prominent exponents.

a, b, after Schiaparelli.

c, d, after Lowell.

e, f, after Maggini.

NOTE.—*a, c*, and *e* show exactly the same aspect of Mars (merid. longitude 0°): so do *b, d*, and *f* (merid. longitude 180°). It will be noted that the members of each triplet differ very widely in detail.

As depicted by him, the disc of Mars was covered with a perfect spider's web of canals, some 700 in all, of an almost rigidly geometrical character. Many of the canals were duplicated (the components, in general, being strictly parallel), and at many places where one canal intersected another appeared a large dark spot (termed an oasis) usually of a strictly circular shape. Upon his charting of Mars Lowell erected and defended an elaborate argument, almost as ingenious and complicated as his canal system itself, demonstrating that the canals were unquestionably planned to serve as an irrigation system, and that they could be nothing else but the work of living and intelligent beings.

Granting the premises, his conclusion seems eminently reasonable. Many of his opponents wasted a lot of ink and paper in laborious attempts to show, as Whewell might have done, that there was no water on Mars; or, at least, not enough to fill the canals; or that the mean temperature on Mars was such that the water would always be frozen, and so on. To my mind, such reasonings, even if sound in themselves, are beside the point. If the canals, as drawn, exist, they are almost certainly artificial. If they are artificial, then, whatever their purpose, they prove that there is intelligent life in Mars.

Actually, many of the *a priori* arguments have now been abandoned. Dr. W. H. Pickering, of Jamaica, who has devoted many years to the study of Mars, and who has organized a systematic and continuous examination of its details by various observers in different countries, has summed up the position thus:*

"I believe it is now time for planetary astronomers to change their views with regard to surface conditions

* *Report on Mars*, No. 30, W. H. Pickering (Mandeville, Jamaica, December 20, 1924).

on that planet, and adopt the position that not merely its temperature, but also its atmospheric pressure, closely resembles that found on the earth.* Heretofore we have doubtless all felt that it was possible that it supported animal as well as vegetable life, and even that intelligent life, if not proved, was not impossible. Now, however, we may perhaps say that with similar conditions to those found on the earth, and vegetable life assured, animal life is almost certain. Furthermore, if it, and if intelligent life exist there, as the straight and narrow canals seem to imply, then the evidence now adduced indicates that it need not be so very unlike ourselves as we have heretofore been led to surmise."

But Lowell's opponents—and a man of such outstanding personality and novel views was bound to make opponents—were (and, for that matter are), on much stronger ground in questioning the objective reality of the canals *as drawn by him*.

We see with the brain, not the eye. The eye is only an instrument, and not a very perfect instrument at that; in fact, Helmholtz once said that if his instrument-maker were to send him an instrument as badly designed as the human eye he would send it back, and decline to pay the carriage. The function of the brain is to put the right interpretation on the messages which it receives from the eye: and interpreters, however competent, are never infallible. It is often very difficult to determine exactly how much we see, and how much of what we think we see is supplied by our imagination; and that is the crucial difficulty in deciding as to the reality of the Martian canals.

* This view has since found further support in observations made with thermo-couple apparatus in 1926 by various observers.

The expert can see more with half an eye than the novice with two. Agassiz, the American naturalist, used to give a student some quite simple fossil to study, and ask him to say when he thought he had seen all he could in it. This usually happened in about ten minutes, after which Agassiz would show him something quite obvious and important which he had overlooked. The process would be repeated until (in about two days) the student had learned to appreciate the difference between seeing and observing.

Similarly, the campanologist can at once detect all sorts of overtones and partial tones in the note of a bell; refinements which the ordinary man fails to appreciate simply because he does not know what he should listen for. And the same, *mutatis mutandis*, holds good of all our senses—the right use of them no more “comes by nature” than, as Dogberry imagined, do reading and writing.

On the other hand it is possible to be over-subtle, and to get into a state of “expectant attention” in which the brain outruns the senses and reads into their messages more than these contain. And, in consequence, while it is true that a novice at areography might fail to see canals on the Martian disc which, to the eye of a trained observer, were staring him in the face, it is no less true that the latter might persuade himself that he saw canals where he actually saw disconnected marks which, he was convinced, formed a continuous line.

There is a well-defined limit, for any person, as to what he can see clearly enough to appreciate its shape correctly. Below that limit one tends to see objects, whatever their real shape, in one of two forms—lines or spots. And it must be remembered that in straining one’s vision—as, even in the best conditions, one must—to

make out the Martian canals, one is putting a considerable tax upon both the eye which receives the impression and on the brain which interprets it. It is not surprising, therefore, if the result is, to some extent, an illusion.

Perhaps I may be allowed, in this connection, to mention an experience of my own. The drawing of Fig. 17 necessitated a good deal of work under conditions somewhat resembling the actual mapping of Mars through the telescope. The original maps which I used had to be re-drawn on the orthographic projection,* which entailed tracing the path of each canal through a set of some four hundred squares occupying an area of about four square inches. Each tracing was then reduced and inked-in, by viewing it through the paper by transmitted light, in a darkened room. The work occupied about a fortnight, and at the end of this period I found that I was beginning to "see canals" in all sorts of places—in the foliage of trees, in the shadows dappling a garden path and, most curious of all, on the smooth and well-lit surface of a billiard table. After a few days' rest, this illusion—undoubtedly a product of eye-strain—disappeared.

The late E. W. Maunder, in his *Are the Planets Inhabited?* † has put the case for the illusory nature of the canals very clearly:

"It is sufficient, then, for us to suppose that the surface of Mars is dotted over with minute irregular markings, with a tendency to aggregate in certain directions, such as would naturally arise in the process of the cooling of a planet when the outer crust was contracting above an unyielding nucleus. If these markings are fairly near each other it is not necessary,

* Schiaparelli's map was on the stereographic projection; Lowell's and Maggini's, on Mercator's.

† London, 1913. Pp. 102, 103.

in order to produce the effect of 'canals,' that they should be individually large enough to be seen. They may be of any conceivable shape, provided that they are separately below the limit of defined vision, and are sufficiently sparsely scattered. In this case the eye inevitably sums up the details (which it recognizes but cannot resolve) into lines essentially 'canal-like' in character. Wherever there is a small aggregation of these minute markings, an impression will be given of a circular spot, or, to use Prof. Lowell's nomenclature, an 'oasis.' . . .

"The above remarks apply to observation with the unaided eye, but the same principle applies yet more strongly to telescopic vision."

Maunder, with Mr. J. E. Evans,* carried out a rather striking experiment in support of his views. A number of boys at Greenwich Hospital School were set to copy what they could see of a design, similar to a map of Mars, pinned to a blackboard. The design was tinted to correspond with the general outlines of the Martian "seas" and "continents," but had no definite lines of any kind on it. The drawings produced by the class varied in a very peculiar manner. The boys sitting nearest the board reproduced the design practically in facsimile. Those at the back of the room did much the same, but of course less accurately. In neither case was anything extraneous introduced. But some of the boys at intermediate distances also thought that they saw (and accordingly drew) lines which resembled the Martian "canals." It should be added that none of them had any knowledge of the question at issue, nor was anything hinted as to what they ought to see. Still, it must be admitted

* E. N. Maunder and J. E. Evans, *Monthly Notices, R.A.S.*, lxiii. 488, June 903.

that, while suggestive in its result, the conditions of the experiment were so different from actual observation that, as Lowell pointed out, the fact that some of the boys were mistaken in what they thought they saw did not prove that he had been.*

Still, it is difficult to escape the conclusion that Lowell's drawings, and those of his assistants and disciples, owe their peculiar and distinctive look to a convention which he adopted (consciously or otherwise); that, in short, he founded a "Lowell School of Areography." I have not seen any of the drawings produced at Flagstaff since Lowell's death in 1916, but much of his influence is undoubtedly to be traced in the Martian drawings of Dr. Maggini, Director of the Royal Observatory, Catania (see Plate VII).† The "spiritual home" of a Maggini drawing is as distinctly Flagstaff as that of a Maggini violin is Brescia. In this connection a remark of Pickering's, as to the Maggini drawings published by him, is very interesting:

"He [Dr. Maggini] says for instance that he could make drawings of Mars closely resembling those of the other observers if he chose to do so. That is to say, their drawings resemble the planet as he sees it. He prefers however to add to the general outlines and canals that he readily sees, certain other features which are extremely faint and difficult, so difficult in fact that in order to show them at all on paper their intensity must be greatly exaggerated—exaggerated out of all proportion to the other detail. This of course destroys the resemblance of the drawings to the planet.

* He afterwards claimed to have proved, by a very similar experiment, that his canals were undoubtedly no illusion!

† It is fair to say, however, that Dr. Maggini has distinctly stated that he does not accept "the Lowellian theories of Mars."

This statement at once explains a good deal, and I believe really solves the question.”*

It certainly explains a good deal, but it must be remembered that Lowell never made any admission of the kind. Yet the history of areography has many instances of structures—at first drawn in all good faith, as Schiaparelli and Lowell drew the canals and oases, as simple, regular, geometrical lines and circles—gradually becoming resolved, by patient examination, into finer and more complex detail. With the gradual advance of telescopic power, it seems not unlikely that even the larger canals, whose existence is undoubted, may be found to present a very different appearance, and that the smaller may be resolved, as Maunder suggested, into irregular markings. E. M. Antoniadi, whose work on Mars with the great 32·7-inch refractor of the Meudon Observatory near Paris is well known, has gone so far as to say:

“The conclusion is that if, by the canals on Mars, we mean straight lines, then they certainly have no existence; but if, by canals, we mean irregular lines of complex structure which have been produced by natural causes, then their existence is undoubted.”

And it may be recalled that while Lowell claimed, in 1897,† to have observed similar canal-like markings on Venus, “perfectly distinct” markings, whose contours “had the look of a steel engraving,” he spoke of the same markings in 1906 ‡ as “hazy, ill-defined and non-uniform,” while later he gave them up as an optical illusion—although, later still, he once more asserted that

* *Report on Mars*, No. 25, 1922.

† *R.A.S. Monthly Notices*, March 1897.

‡ *Mars and its Canals*, pp. 178 f.

they were objective! Again, Dr. A. E. Douglass, who was Lowell's chief assistant at Flagstaff from 1894 to 1901, stated in an article published in 1907 * that he considered many of the faint canals mapped at Flagstaff to be illusory. It may be added that Pickering has shown that, by using a very low power, it is quite easy to "see" canals on the surface of the moon.†

One might think that if, as shown by Plate VII., there is so much difference in the drawings made of Mars, under exactly the same aspect, by different observers, and if there is, in consequence, so much variation in the maps of Mars based on those drawings, the obvious course would be to have recourse to photography. This has been done, but unfortunately the results (or, at least, such results as are generally accessible) are almost as difficult to interpret as the Martian disc which they depict.

The first photographs claiming to show the canals were, appropriately enough, taken at Flagstaff. After various unsuccessful attempts, Mr. C. O. Lampland, using a colour screen and with the aperture of the telescope reduced by a diaphragm, succeeded in obtaining a number of photographs, of which six were published in the *Lowell Observatory Bulletin*, No. 21.

To judge by Lowell's accompanying remarks, headed

"THE CANALS OF MARS—PHOTOGRAPHED,"

the existence of the canals had been triumphantly vindicated. He says:

"... The negatives thoroughly confirm the eye in showing not only the existence of the canals but the fact that they are continuous lines and not a synthesis

* *Popular Science Monthly*, May, 1907.

† *Report on Mars*, No. 6.

of other markings. Beyond a certain magnification, if a magnifier be used to examine them, the grain of the plate will show. This must not be taken for discontinuity in the image.

Two points are worthy of notice:

1. The corroboration of the fact of the canals by the photographs.
2. The corroboration of the methods found most efficient to their detection visually.

PERCIVAL LOWELL."

But at first sight it is difficult, on examining the photographs, to take them seriously.

To begin with, they are quite minute. The disc of Mars is just a quarter of an inch across. Three such discs, arranged clover-leaf fashion, could be covered by a threepenny-bit.

Secondly, they are not at all sharp in definition, and four of the six so dark that it is difficult to make anything of them.*

Thirdly, while on the other two it is certainly possible to detect the main outlines of some of the larger Martian continents—if such they be—it is utterly impossible to detect anything remotely resembling a canal. In view of the scale, no one of ordinary intelligence would dream of looking for such a thing. And yet Lowell claimed, quite seriously, that eight canals and an oasis ought to be recognized with the aid of a map of Mars. What was really needed was a pair of very rose-coloured spectacles.†

* It should be noted that they are actual positives, and not reproductions by any mechanical process.

† I think I ought to say that I have examined these photographs carefully, both with and without a Coddington lens. I do not need glasses, and I will back myself at any time to write the Lord's Prayer in capitals, without a magnifier, on a disc considerably smaller than a threepenny-bit.

Larger and better photographs have since been taken at Mount Wilson, and at the Yerkes and Lick Observatories; but on those which I have been able to examine there is no vestige of any canals.* One could not expect to see them, for on the largest photograph the Martian disc is less than an inch in diameter; and, of course, they will not stand much enlargement. Lowell's, by the way, were enlarged 1.4 times, so that on the plate the diameter of Mars would have been 0.14 inches only.

It is fair to say, though, that photographs which actually show at least some of the canals have recently been taken at Flagstaff, although I believe that these have not yet been published. I had some correspondence on this point a short time ago with an American friend, and subjoin a few extracts.

(*J. S. to R. T. G.*, 24.11.1928.)

"I have seen these photographs,† and there is no doubt that the canals (when I say 'canals' I really mean 'the markings that we call "canals"') are there. But the best photographs of them are those made at the Lowell Observatory, in Arizona. I have been there several times, the latest being but a few months ago, and Dr. Slipher and his colleagues have shown me their actual negatives. . . . I think that my experience is especially significant because I firmly believed them to be due to optical illusions until I made my first visit to Flagstaff in 1923, and was not convinced of their objective existence until I saw the negatives.

So far as Lowell's interpretation of them goes I am

* There are certainly one or two streak-like markings, possibly 3-400 miles wide or so—but nothing which in the least resembles the straight, narrow spider-lines depicted by Lowell.

† The Lick photographs.

still quite sceptical, for I think it quite likely that these markings have some natural origin. . . .”

(R. T. G. to J. S., 19.12.1928.)

“Did the negatives you saw show any duplication? I ask this, because to my mind the duplication is the *experimentum crucis*. And, if you could identify any particular canal in different positions on two or more negatives, did it always appear straight, or did it curve, as a natural marking should do, to an amount varying with its distance from the centre of the disc?”

(J. S. to R. T. G., 18.1.1929.)

“As regards the ‘gemination’ of the canals, I must say that I have not seen this on any negatives. I have seen the statement that they have been photographed in this condition, but I am unable to verify this from my own experience. Nor could I say that the photographs of the canals show them curved at the side and straight in the centre as a real marking on the planet should be. However, the canals can only be photographed well, it appears, when they are in the centre of the disc. The greater thickness of the planet’s atmosphere seems to obscure them when they are near the edge, so that I doubt if this test could be applied very conclusively. Of course, the drawings show them curved at the side as they should be, and I, for one, am willing to accept as accurate these drawings by experienced observers.”

No doubt, many others will say the same, and quite justifiably so. But, as previously explained, the trouble is that against such drawings can be set a very large number of others whose provenance is equally un-

assailable—and which, while they depict the planet under precisely the same aspect, show no canals at all.* The existing body of evidence from drawings, while it is insufficient to settle the existence of the canals, speaks with no uncertain voice on at least one point. And that is, that such evidence is entirely unreliable. The drawings, pro and con, cancel each other out, and any conclusion based upon them is really determined by the selection of the drawings to which one decides to pin one's faith. It is this regrettable, but not incomprehensible, fact which makes it so vitally necessary to obtain, if possible, clear photographs of at least a good many of the canals.

It is not, as a matter of fact, absolutely certain that, with our present photographic methods, we should be able to say with confidence that if a canal appeared as such on the plate, it was so in reality. It seems possible that, when dealing with faint detail not far from the limit of visibility, detached spots may run together, so to speak, in the lens as they do in the eye; and there is, moreover, the grain of the emulsion to consider. It seems possible that we may be driven back to taking our photographs on silver, after the manner of the old daguerreotype; but, unfortunately, a long exposure (such, for example, as is used for photographing faint stars) is out of the question by reason of Mars' rotation. However, the problem is one which we may legitimately hope to see solved in a comparatively short time; and, whether the famous canals are proved to be real, or partly real and partly illusory, or wholly illusion, there can, I think, be only one opinion as to the skill and devotion shown by the many astronomers who have already done so much towards the solution of one of the most fascinating of the many astronomical enigmas.

* See Plate VII.

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